The Dissertation Committee for Carlos Dominic Esparza
certifies that this is the approved version of the following dissertation:

Essays on Education and Religion

Committee:

Leigh Linden, Supervisor

Manuela Angelucci, Co-Supervisor

Richard Murphy, Co-Supervisor

Joseph Kaboski

Essays on Education and Religion

by

Carlos Dominic Esparza

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Essays on Education and Religion

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Carlos Dominic Esparza, Ph.D.
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Supervisors: Leigh Linden
Manuela Angelucci
Richard Murphy

This dissertation empirically explores how external influences in both the education and religious spheres affects human capital accumulation. Chapter 1 explores the effects of people's attachment to religion on religious participation and on human capital by studying the sexual abuse scandal within the Catholic Church. Reporting by major media outlets in 2002 revealed a staggering amount of sexual abuse by clerics and its ongoing cover-up in the United States by the Church's hierarchy. This news shook many of the faithful's trust in the religious institution and led to a crisis of faith. The intensity of the scandal, as measured by the number of allegations against clerics, differed across the dioceses in the country. Exploiting this variation with a difference-in-differences strategy, I find that dioceses with a high number of allegations experienced diminishment in demand for religion across several different measures. Parents are less likely to have their children receive sacraments and

attend parochial schools. Parish membership declines by more than 7.1 percent, and student enrollment falls by 13.2 percent.

Chapter 2 further investigates how the abuse scandal affected human capital accumulation. The crisis had effects that extend beyond religious attachment. I present evidence that the clerical abuse scandal leads to worsening mortality rates related to deaths of despair, i.e. deaths related to suicide, accidental drug overdose, or liver disease. In particular, at the county level there is an increase in the incidence of high mortality rates of respectively 1.2 percent and 1.8 percent for accidental overdoses and suicides for middle aged men.

Chapter 3 is joint work with Andrew Lee. We study the causal impact of noise pollution on educational outcomes of students attending public institutions in the state of Texas. We research this through the setting of wind farms, whose turbines are a source of low-frequency noise pollution. Using geolocation information, we identify which schools lie near wind farms. Under a difference-in-differences framework, we compare schools that are in the vicinity wind turbines with similar schools that are not. Using publicly available data on public schools, we estimate the negative impact of noise pollution on academic achievement. The installation of wind turbines leads to 0.046 standard deviation reduction in reading scores on standardized tests, and there are heterogeneous effects among certain subgroups of students.

Table of Contents

List of Tables

List of Figures

Chapter 1

How do Abuse Allegations Affect the Religious Participation of Catholics?

1.1 Introduction

One would be hard-pressed to deny the influence that faith has had on both cultures and societies. Religious institutions have had varying impacts across key areas of civic life. For instance, whole education networks, hospital systems, and charitable organizations are sponsored (if not run) by religious entities. At the same time, faith plays an important role in many individuals' lives. As of 2014, 77 percent of Americans are religiously affiliated with 55 percent praying daily and 36 percent attending weekly services (Pew Resarch Center (2014)). Even with this prevalence, religion remains under-researched in the economic literature.[1]

The Catholic sexual abuse scandal provides an opportunity to add to this relatively nascent literature and study how people react to a crisis of faith. In 2002, *The Boston Globe* broke the major story about sexual abuse of minors by clergy within in the Catholic Church. What started locally in the Boston area quickly spread throughout the United States. This ripple effect led to the number of allegations against priests to skyrocket. In a relatively short time, people learned not only about the abuse allegations but also about an extensive cover-up of these allegations by the hierarchy in the Catholic Church. This negative news led people to lose trust not only in the religious institution but also in the faith leaders who they had held up as being closest to God. Simply put, many Catholics questioned their faith. Beyond the general national news

[1] Iyer (2016) reviews the literature and shows that in recent years there has been an increasing number of articles and interest in the economics of religion.

coverage, areas had different exposures to this scandal due to number of local offenders and due to the varying responses of the local Church leadership along with the perceived levels of its complicity to the scandal.

For a variety of reasons, this crisis of faith could move people to respond in ways that would decrease the demand for religion and affect human capital outcomes. First, one could hypothesize that the negative publicity of the abuse and associated cover-up might lead people to become disillusioned and leave the religion. Additionally, one would expect parents to pull their children from early religious education programs and schools to protect them. Third, in Chapter 2 of this dissertation we also explore how this exploitation of minors and a violation of the people's trust may have cut off a lifeline where people had social, emotional, and psychological support.

My econometric strategy to examine how this crisis of faith affected the Catholic faithful is to exploit the variation in the exposure of the scandal, as measured by the number of public allegations, across dioceses, i.e., church juridical regions, in a difference-in-differences framework. I compare dioceses that had a high number of allegations in 2002, specifically those in the upper quartile, to those that did not.[2] To do this, I obtain allegation information from the nonprofit Bishop Accountability, and I utilize a new set of participation data from the *Official Catholic Directory*.

[2]This threshold for the number of first-time accused clerics is greater than five. The results of this chapter are robust to setting a higher or lower threshold as shown in Appendix A.

First, I find that demand for religion declines. Religious participation decreases across several different measures. The number of Catholics connected to a parish church decreases by 7.1 percent, and infant baptisms drops by 7.9 percent. This totals to about 2.5 million less parishioners for the fifty treated dioceses, or about 14.1 percent of the increase in the number of ex-Catholics since 2000 as measured by the Center for Applied Research in the Apostolate (CARA).

Second, school enrollment, both a secondary measurement of participation and an indicator of human capital accumulation, suffers. The number of students who attend diocesan sponsored elementary schools declines by 13.2 percent and high school enrollment by 17.1 percent. Together this is nearly 180,000 students which represents 60 percent of the total decline in all private school enrollment over the past two decades in the US. Additionally, while the demand for Catholic diocesan schools drops, the effect on other, both Catholic and non-Catholic, private schools differ. For instance, nonreligious private schools experience an increase in high school grade level enrollment.

This study contributes to the larger literature on the economics of religion. In particular, the failures of the institution of the Catholic Church led people away, and I show that this change in the faith experience of Catholics is linked to larger issues affecting human capital outcomes like education and health. This is in a similar vein to Clingingsmith, Khwaja and Kremer (2009) which shows that the Hajj can be life changing for those who make the pilgrimage. Along the same lines, papers have shown other ways that religious

institutions affect not only the demand for religion but also the well-being of people. For instance, religious leaders can influence followers behavior (Bassi and Rasul, 2017), and missionary work has long lasting, i.e., multigenerational, impacts on human capital ((Valencia Caicedo, 2018); (Calvi, Hoehn-Velasco and Mantovanelli, 2020)). Catholic education systems also have long term effects. Papers find that students at Catholic high schools have higher graduation rates and a greater chance to attend and finish college ((Evans and Schwab, 1995); (Neal, 1997)).

This work contributes to an existing literature studying the impacts of the scandal. Studies have shown that the scandal negatively impacted religious giving, religious employment, and Catholic education ((Hungerman, 2013); (Bottan and Perez-Truglia, 2015); (Dills and Hernández-Julián, 2012)). The link between religiosity and state welfare has also been examined (Dills and Hernández-Julián, 2014). My paper complements these studies by bringing direct measures of religiosity and religious practice, such as parish membership, churches, and sacraments, from the previously non digitized data of the *Official Catholic Directory* and a new dimension of detail at the level of schools to bear on this question. I find that the response differs among the types of Catholic schools and other private schools. Overall, the contribution of this chapter describes how a crisis of faith hurt participation in both the liturgical and educational spheres of the Church.

1.2 Background

1.2.1 The Scandal

In a pastoral letter, the Most Reverend J. Douglas Deshotel, bishop of the Diocese of Lafayette, recently lamented that the public first confronted the reality of the clerical sexual abuse back in 1984 when a Louisiana priest was accused and eventually plead guilty to molesting more than 30 children. Scandal would resurface again in the 1990s when some dioceses had to pay out millions of dollars in settlements.[3] Even with these events, the scandal did not rise to the forefront of the public's consciousness until 2002 when *The Boston Globe* published the exposé on sexual abuse of minors by clerics. What prompted this investigative reporting was concern about actions taken by Church officials in response to a series of accusations of sexual abuse made against John Geoghan, a Boston priest. Bishops had allowed the priest to serve in many parishes despite his checkered history.

This reporting was a watershed event. There were growing reports throughout the United States about priests who sexually abused minors, and the news coverage correspondingly increased. The number of allegations against priests skyrocketed. The John Jay Report, commissioned by the US bishops, found that in 2002 dioceses received notification of 3,399 incidents of past

[3]For instance, an important case dealing with scandal before the Boston crisis dealt with the former Diocese of Dallas priest, Rudy Kos. In 1997 a lawsuit against the diocese and Kos, a jury initially awarded $120 million to victims, but the case was eventually settled on appeal with the diocese agreeing to pay $23.4 million. Kos was convicted and sentenced to life in prison in 1998.

abuse. While this scandalous behavior of priests had been documented before in isolated incidents[4] across the country which resulted in some dioceses having to pay out millions of dollars in settlements, what became apparent was that the hierarchy of the Church had previous knowledge of many of these accused priests.[5] The public had been aware of past singular incidents of abuse, but now a case could be made that the Church hierarchy was complicit or at least ineffective in how it handled abuse cases. Discoveries were made that bishops moved priests around after priests underwent psychological treatment, and they did not report offenses to legal authorities. A case could be made that the leadership concealed the truth in order to avoid negative publicity.

For many, the scandal was more than just a small group of clerics who had done the most abominable things to innocent children. What was just as repulsive was the appearance that those who were entrusted with their pastoral and spiritual care, i.e. the bishops, had acted recklessly by keeping credibly accused priests in ministry and seemingly had protected the abusers. The faithful's trust was shaken in those who they saw as closest to God. It is understandable that this scandal would lead some to doubt their religion's leaders and, in general, to enter into a crisis of faith.[6] Anecdotally, there

[4]The John Jay Report estimates that only about 10 percent of the eventually reported incidents of abuse from 1985 and before were reported to dioceses by 1985. The 2002 scandal brought several new allegations from the past to the forefront.

[5]In fact, journalists at *The Dallas Morning News* found at the time of the scandal that nearly two-thirds of US bishops had let accused priests to continue working after allegations were made.

[6]For instance, Jones (2019) reported that 22 percent of Catholics polled questioned whether to remain in the Catholic Church after the 2002 scandal.

was much anger in the pews, and some people quit attending Sunday mass altogether.

While, at the national level, the whole Church was indicted by this scandal, there were local differences in the exposure of this crisis. There was widespread spatial variation in where the allegations were made against priests. Certain areas of the country, like the Northeast, were more barraged with accusations than other parts. The faithful depending on what diocese they lived in had different intensities of exposure to the scandal.

1.2.2 The Church in the United States

The Church is separated into geographic, juridical areas known as archdioceses or dioceses.[7] In the United States, there are 176 territorial dioceses. Currently, each state has at least one diocese while Texas has the most with fifteen. Furthermore, only four dioceses cross state borders.[8]

While some dioceses have more than one bishop, each is headed by a single bishop, who is also known as the ordinary. The ordinary has as his charge the pastoral and spiritual care of all the Catholics who reside in his diocese. To assist in this ministry, the bishop ordains priests and deacons and

[7] An archdiocese is usually just a diocese with a larger catholic population than other dioceses in the region. For the purpose of this chapter, diocese will refer to both a diocese or an archdiocese.

[8] The Diocese of Gallup lies in the states of Arizona and New Mexico, the Diocese of Wilmington traverses the Delaware-Maryland border, the Diocese of Norwich in Connecticut has a small portion in New York, and the Archdiocese of Washington covers the District of Columbia along with a portion of southern Maryland

assigns them to their apostolic ministries, usually church parishes. The bishop is the only one who has the direct authority to move priests around within their own diocese. As mentioned above, bishops took criticism for moving around abusers from place to place.

A typical diocese is divided up into parishes so that the local church can more easily meet the pastoral and spiritual needs of its members. The norm is that each parish church is headed by a pastor appointed by the bishop who is in charge of caring for those who live in the parish boundary.[9] The usual term of office for a pastor is six years, but it can be renewed by the bishop. However, a church member could come into contact with several different priests that serve the diocese over the years even if she never moved parishes. The overwhelming majority of parish boundaries are geographic and determined by the Catholic population in the area. As such, the boundary of an urban parish may be only several square city blocks while the jurisdictional reach of a rural parish could stretch several square miles.

Whom the bishop assigns to a parish church is of utmost importance because the parish is the center of a practicing Catholic's faith experience. A Catholic can attend Sunday and, possibly, daily mass, take religion classes, receive counseling from a minister, perform social outreach, and socialize all at the local church. Key to this faith experience are rituals centered around life events, and the most important are the Sacraments of Initiation.

[9] Additionally, some parishes may have parochial vicars, or associate pastors.

The Sacraments of Initiation are received when someone enters the Catholic faith. They include Baptism, First Communion, and Confirmation. Baptism is the first sacrament that an individual receives when one comes into the Catholic Church. While people may be baptized if they convert later during adulthood, it is the norm that infants are baptized within the first year after birth. In the United States, it is usually the case that First Communion chronologically follows Baptism. Baptized children are able to receive First Communion once they reach the age of seven, and they have the required preparation. After receiving this sacrament, the Catholic can fully participate in the mass. Finally, Confirmation is typically the last sacrament that a minor receives. Depending on the local diocese, children might receive this sacrament while they are in elementary school. However, the vast majority of dioceses have teenagers preparing to receive this sacrament when they are in middle school or high school. Upon receiving Confirmation, the person is fully initiated into the faith.

Another fundamental way that people experience the Catholic Church is through the Catholic school system. Enrollment informs us about religious participation because the faithful are encouraged to send their children to Catholic schools. In fact, it is a common practice that families who attend a parish church receive a discount on tuition at that particular parish's parochial school. Catholic schools not only educate their students according standard curricula but also aim to form them to be disciples and practitioners of the Catholic faith. Common practices during school time include theology classes,

weekly mass or prayer services, and sacramental preparation for first reconciliation and communion.

While all of these ways definitely speak to religious participation, looking at school enrollment also says something directly about the market for private schools. In particular, learning about how people react to the scandal in regards to their choice in education informs us about the demand curve for Catholic and other private schools. The predicted decrease in demand has ramifications that potentially extend beyond the sphere of religion. When parents choose to forgo education in the Catholic school system for their children, one may question how this will affect the accumulation of human capital, not only in standard skill and knowledge acquisition but also in the many dimensions of human formation which religious schools emphasize.

Even with the similarity of mission, in the United States, there are generally speaking two different types of schools within the Catholic school system. The first, diocesan schools, are governed and receive curriculum direction from diocesan officials. Parochial schools, usually serving students in kindergarten through eighth grade, are tied to a particular parish, and the pastor has at least a minimum direct care of the school through the hiring of the school principal. Diocesan high schools tend to be regional. The other type, non-diocesan private Catholic schools, have a much more varied background. For instance, some of the schools are governed by a private lay board while others are sponsored by a religious congregation, such as the Jesuits. Under these circumstances, these schools operate under the auspices of local dioce-

ses, but a diocese has no direct control of the school. These schools get their institutional identity not only by being Catholic but also from the charism of their sponsoring group. It seems plausible that the abuse scandal could affect enrollment differently at these types of schools.

1.3 Data and Methodology

1.3.1 Data

The main identification strategy relies on information gleaned from the non-profit Bishop Accountability, incorporated in Massachusetts. The mission of this organization is to hold the church leadership accountable for the handling of abuse allegations. Key to this is the transparency of these allegations against Catholic Church officials, including bishops, priests, deacons, brothers, and sisters. The organization provides a diocesan list of each church official who has been accused of sexual abuse of a minor along with supporting public documentation of the allegation, including court documents, diocesan press releases, and news articles. As such, one can construct a data set, that has for each accused cleric the date that an allegation was first publicly known. From this, I can tally by year and by diocese the number of first-time allegations independent of when the alleged abuse occurred.

In conjunction with the allegation data, the *Official Catholic Directory* (OCD) is a primary source of data. Published annually by P.J. Kenedy and Sons, the OCD holds a wealth of information, including parish assignments, institution listings, individual priest assignments, and general statistics for

each Catholic diocese in the United States. To my knowledge, I am the first to use this complete data source. For this analysis, I use data from 1991 to 2016. Key variables of interest deal with Catholic participation and identity. In particular, the OCD has information on the Catholic population, the number of Baptisms, First Communions, Confirmations, and enrollment figures in religious education programs and Catholic schools in each US diocese. Additional diocesan school enrollment data for 1997-2018 comes from the National Catholic Educational Association (NCEA). I complement this with the biennial Private School Universe Survey (PSUS) from the National Center for Education Statistics (NCES) for the years 1991-2016 which has grade level enrollment at the school level.

Intercensal demographic county level data, including race and age, comes from the US Census Bureau. Additionally, county level data on personal income comes from the Bureau of Economic Analysis (BEA). County level data is aggregated to the diocesan level by using a county to diocese cross walk.

1.3.2 Allegations and Treatment

In order to have a measure of the exposure of the sexual abuse scandal, I count the number of allegations that each diocese has. To have better understanding of this metric, it is best to first consider the type of allegations that will be counted. Similar to Hungerman (2013) and Bottan and Perez-Truglia (2015), a sexual abuse allegation is counted if it is publicly leveled against

a Catholic Church official, such as bishop, priest, deacon, brother, sister, or seminarian. An additional requirement is that the allegation has to be the first-time that the official has been publicly accused. If a priest has served in multiple dioceses, the diocese where the accusation takes place is considered the primary diocese of record in the data. Furthermore, the year when the allegation is first made public (and not when the abuse occurred) is when one expects the negative news to effect the diocese. Under this framework, public allegations made in 2003 and 2005 against a priest that occurred in 1975 while he worked at a school in diocese x counts as an allegation for the diocese x in 2003 only. Finally, this chapter does not distinguish between allegations that eventually result in settlements or convictions, those that are deemed credible, or those not considered unsubstantiated by Church officials. This measure relies on the negative shock that results from the public revelation that a Catholic official has been accused of sexual abuse.

Figure 1.1 presents the tally of first-time allegations levelled against Catholic clergy and religious for 1991 to 2018. As one might expect, the number of allegations are very low leading up to the 2002 reporting by *The Boston Globe*. At that time, the US Catholic Church experienced a spike in first-time public allegations. In subsequent years, the number of allegations dropped but the level was higher than the years prior to the 2002 scandal. This year clearly stands out as when news about the sex scandal was publicly made known.[10]

[10] Another spike also occurs in 2018 which is a result of a new round of allegations resulting

This study exploits the difference in intensity of the publicity of the abuse scandal. I measure the intensity by counting the number of first-time allegations in each diocese that occurred in 2002 when news of the national scandal first broke.[11] One might wonder what is the impact of the scandal across the United States. As news spread, many people were affected, and anecdotally many people left the faith. This project is unable to measure this aggregate effect of the scandal; rather, it looks at the local effect of a diocese's susceptibility to the scandal on participation, i.e. the relative effect by comparing high allegation to low allegation dioceses.

I consider dioceses in the upper quartile of the number of allegations in 2002 as treated. The cutoff is more than five first-time allegations in 2002. Under this standard, fifty locales are considered high allegation dioceses. Figure 1.2 delineates the borders of the dioceses in the continental US and shows the high allegation and low allegation dioceses, and Table A.1 provides the

from two major news events that happened in the summer of that year. First, allegations against the former archbishop and cardinal, Theodore McCarrick, were deemed credible by the Archdiocese of New York in June. Second, the report from the Pennsylvania grand jury's investigation into Catholic Church sexual abuse was released in August. A host of previously undisclosed allegations against priests were made public in the report. These two events led to a nationwide call for transparency. Dioceses and religious orders released historical lists of priests who had credible allegations leveled against them. The first of these entities began to roll out the credibly accused lists in the fall of 2018 and others followed suit throughout 2019. There are still some dioceses who have not released lists.

[11] The total number of allegations seems like a reasonable gauge of intensity because the publicity from even one allegation could be broadcast throughout the diocese by any number of media outlets. Yet, one may argue that the exposure of the scandal does not depend on the total number of allegations and suggest that the number of allegations per capita would be a better measure. When the chapter uses the number per person, the results qualitatively hold and are included in Appendix A.

number of allegations in each of the treated dioceses in 2002. The analysis and results which follow in this chapter qualitatively hold when the cutoff is relaxed or tightened, and this is discussed in Appendix A.

Dioceses that have a much longer established history are more likely to have a large number of allegations. Many in the Northeast experienced a flood of first-time allegations in 2002. Additionally, those with the most allegations tend to be those with the largest population. The dioceses of Baltimore, Boston, Chicago, Los Angeles, New York, and Philadelphia comprised about 21 percent of the US Catholic population in 2002. Additionally, the spread of the scandal reaches some Midwestern and Western dioceses. Table 1.1 provides summary statistics by high allegation and low allegation dioceses. The average high allegation diocese is larger than the average low allegation diocese on a number of key characteristics.

Inspecting how the number of allegations evolve between high allegation and low allegation dioceses, high allegation dioceses on average have more first-time annual allegations post 2002 than the low allegation dioceses. Figure 1.3 presents this information by year and treated group. The dashed line represents the average cumulative number of first-time allegations for high allegation dioceses while the corresponding solid line for low allegation dioceses. Before news of the scandal, high allegation dioceses had a slightly higher number of cumulative allegations. At the scandal in 2002, there is a steep jump and relatively much smaller one for low allegation dioceses. The stark difference in cumulative allegations between the two types of dioceses is maintained

throughout the period of this study's analysis.

Due to the sudden jump in allegations, one might imagine that people react to the news of the scandal immediately. However, there is a distinct possibility that people may wait due to the way they process the information of the news. First, they may exhibit a confirmation bias where they might be slow to update their opinion about the Church. For instance, an individual could chalk up news reports of the scandal to media bias.[12] Second, a person may employ a heuristic of optimism. Individuals may be wary to think something so horrible could occur in their beloved religion. Under these circumstances, repeated news of allegations would budge someone to change their belief about the Church. Due to the cumulative effect of the negative publicity that continued after the initial news in 2002, we may expect to see both delayed and lingering effects of the crisis on participation variables.

1.3.3 Empirical Strategy

The intensity of the sexual abuse scandal varied across different dioceses within the Church; as such, I can exploit that variation in a difference-in-differences framework. In order to measure the intensity of variation, I count the number of allegations as mentioned in the previous section. I have shown

[12]In fact, Mancini and Shields (2014) through a 2010 national survey find that increased exposure to media reporting of the scandal is associated with increased public confidence in the Church in being able to handle future sexual abuse cases. They argue that this may be do to a boomerang effect which suggests that continual negative attention to one issue might backfire in shaping public opinion. In other words, people may question the bias of the reporting and form a defensive stance on the issue.

that the number of allegations vary across dioceses. This number of new allegations seems to be something that should be correlated with the scandal in a particular locality. For example, I assume that more allegations would be associated with more news reports and other public revelation clergy sexual abuse. What this chapter then does is test for whether or not dioceses that had a greater exposure to the scandal are related to the outcomes of interest: religious participation and school enrollment.

The chapter estimates the causal impact of the scandal on the outcomes of interest using the estimating equation for the preferred specification that follows:

$$Y_{dt} = \beta_0 + \beta_1 Treat_d \times Post_t + \beta_2 X_{dt} + \eta_d + \nu_t + \varepsilon_{dt} \tag{1.1}$$

Y is the diocesan level outcome for diocese d in year t, such as the log number of infant baptisms. For this chapter, the preferred specification will use the log of variables.[13] $Treat_d$, an indicator variable, is 1 if the diocese d was a high allegation diocese in 2002. $Post_t$, the other indicator variable, is 1 if the year t is 2002 or later. β_1 is the coefficient of interest that will give the differential effect of having a experienced a higher volume of allegations in the diocese on the chosen outcome. X_{dt} is a vector of diocesan level specific characteristics, such as Hispanic percentage or per capita income. η_d is the difference in outcome due to diocesan effect, ν_t represents the common shocks

[13] Other specifications include per capita, levels, and an index level. The index is calculated as a ratio between the level in year t to the level in 2001 immediately before the abuse scandal, i.e. $Y_{dt} = \frac{level_{dt}}{level_{d,2001}}$. Results for the per capita measure are included in the robustness section.

to all dioceses for a specific year, and ε is the error term. This specification, while primarily for the diocesan level, will be modified for relevant county and school level outcomes.

The event study for the various outcomes of interest comes from modifying equation 1.1:

$$Y_{dt} = \beta_0 + \sum_{T \neq 2001} [\beta_{1T} Treat_d \times \mathbb{1}(t = T)] + \beta_2 X_{dt} + \eta_d + \nu_t + \varepsilon_{dt} \tag{1.2}$$

The treatment variable $Treat_d$ is interacted with a set of indicators, $\mathbb{1}(t = T)$, for each calendar year. As such, the yearly coefficients of interest are the β_{1T}s. I normalize $\beta_{1,2001}$ to zero, so all coefficients are interpreted as changes relative to the year before the news of the scandal broke. The rest follows as above.

In order to make this functional, I made some assumptions about the model. Knowing that some of these are not testable, I conducted several robustness checks to try other assumptions that also seem reasonable. With these, the chapter gets qualitatively similar results. They are further detailed in Appendix A. First, I find that the results are robust to contracting and expanding the threshold for a treated diocese, showing that the results do not come from cherry picking. The preferred specification includes approximately the upper quartile of dioceses in terms of number allegations. This check includes thresholds that look at the top fifteen, twenty, and forty percents of dioceses. Second, the findings are also robust to changing the specification of the outcome variable. Instead of the log of participation variables, the chapter uses outcomes per one million people. Third, I employ a parametric

event study that controls for linear pre-trends, and qualitatively similar results hold. Finally, I use kernel propensity score matching on covariates to get more comparable groups between the high and low allegation dioceses.

1.4 Results: Participation

1.4.1 Sacraments of Initiation

This study looks at the effect of the crisis on religious demand. I am able to get at a more direct measurement of religious participation by utilizing the OCD data than previous literature. I have digitized a unique data set which provides the number of people who have received the Sacraments of Initiation, which are Baptism, First Communion, and Confirmation, in each diocese. Each of these give a unique perspective into the way that the sex abuse scandal has had on the Catholic faithful's religious participation.

Baptism is the first sacrament that someone receives when one comes into the Catholic Church. It is the norm that Catholic families have their children baptized within the first year after their birth. As such, the parents and the godparents profess the faith of the infant in the Baptism ritual. One expects that parents wary from the abuse scandal to avoid baptizing their newborn infants.

Using similar reasoning, one would expect a drop in the number of children who receive First Communion. Baptized children are able to receive First Communion once they reach the age of seven. One of the requirements is that a child receives sacramental preparation through her parochial school

(if she attends Catholic school) or through the parish's religious education program. If students are not enrolled in Catholic elementary schools, parents would have to take the initiative to enroll their children in religious education classes to receive this sacrament. Again, this chapter conjectures that parents would be less willing to have young children involved in these church programs.

Finally, a minor is confirmed only after being baptized and usually after receiving First Communion. In some cases, one may be confirmed at an early age around the same time as when one receives First Communion. For most of the dioceses in the United States, the norm is that children prepare for Confirmation during their teenage years while in middle school or high school. Initially, one would also expect that there would be fewer who would want to be confirmed because of the scandal.

Overall, we can say that each of these measure religious participation in unique ways. Baptism captures purely the attitudes of the parents. Second, First Communion may additionally capture some concern about their children's welfare during the actual participation for reception of the sacrament. Finally, Confirmation captures both of these concerns but also reflects a child's attitude.

Figure 1.4 provides event study plots derived from Equation 1.2 of the effect of the scandal on the log number of infant baptisms on the left in Panel A and on the log number of First Communions in Panel B. The difference in how the outcomes evolve by diocese type is evident in the respective coefficient plots. In the immediate years prior to the scandal of 2002, the coefficients for

the infant baptism regression are close to zero. In the years following, the estimates become negative almost immediately and are statistically significant within five years of news of the scandal. The results are qualitatively similar for First Communion. The delay in the effects possibly may be attributed to four reasons. First, there may be a cumulative effect. While the crux of the scandal occurred in 2002, high allegation dioceses continue to have more first-time allegations than the control dioceses in the few years afterwards. Parents may not have updated their beliefs until after an accumulation of negative publicity. Second, the scandal could have driven young people away from the faith more easily, but it takes some time for these cohorts to become the dominant cohorts having children. Third, the data in the OCD is static as it relies on the administrative reports of the dioceses that are reported in lags. Finally, with regards to the age requirement, we would expect the larger effects to happen beginning seven years after the scandal. In this case, this captures all those who were not baptized initially and who now cannot receive First Communion.

The estimates based on Equation 1.1 of the effect of the abuse scandal on these sacraments is provided in Table 1.2. Results without incorporating diocesan controls are presented in the first and third columns. These estimates are negative and statistically significant. The magnitudes of the estimates, in the second and fourth columns slightly decrease when I include controls, which include the percentage of population that is Hispanic and per capita income for both the infant population percentage for Baptism. Yet, they

remain statistically significant. With controls included, I interpret that the estimates imply that dioceses that had a high number of allegations in 2002 experience a 7.9 percent decline in infant baptisms and a 7.8 percent decrease in First Communions. This amounts to 843 less infant baptisms and 698 less First Communions for the average high allegation diocese based on the pre-scandal means of these dependant outcomes. This evidence shows that parents increasingly kept their infants and young children away from these rituals and incorporation into the Catholic Church.

The fifth and sixth columns of Table 1.2 present the estimates of the effect that the 2002 scandal has on the log number of Confirmations. The magnitudes are much smaller than those of the other Sacraments of Initiation, and they are too noisy to definitively conclude their sign. Why would this sacrament, one in which minors predominantly receive, not experience the same declines as the other initiation sacraments? Since the recipients are usually older when they receive this sacrament, it seems sensible that both the potential recipients and their parents have spiritually and culturally invested more into the Catholic faith. As such, this chapter posits that they would be less likely to end their training for this sacrament, which is key to their formal relationship to the Catholic Church. Additionally, similar to First Communions, one needs to be baptized in order to be confirmed. The long term effect of the scandal may be only realized several years after 2002 if the main driver is children not being baptized. Additionally, the farther out from 2002, one could expect the treatment to get much noisier because people could

move and change dioceses. These constitute some of the possible reasons why there is not a discernible effect.

1.4.2 Catholic Population

Another key statistic for religious participation is the number of Catholics who are connected with a church parish through formal registration. The OCD reports the diocesan Catholic population based on this standard. This metric provides an overall sense of families and individuals who choose to register with their local parish. However, this variable does not include those who never register and still attend Sunday mass or participate in other parish sponsored activities. Additionally, it takes time for a parish to update their membership list when people move to another area or simply stop going to church. Finally, as mentioned before, due to Confirmation bias and general optimistic thinking, some marginal Catholics might be slow to update their beliefs. As such, departures most likely will be realized in the data after a lag.

A time series of the evolution of the US Catholic population in the 176 geographical Latin Rite dioceses along with its corresponding proportion of the total US population is presented in Figure 1.5. Over the last four decades the, the Catholic population, represented by a solid line, has increased from around 47.8 million in 1980 to about 68.0 million in 2017. In the years preceding the 2002 abuse scandal, the population increased at a relatively constant rate to 61.8 million. Looking at the graph, it is evident that the slope of the population curve has decreased in the years afterward. In fact, just as Hungerman (2013)

noted, immediately following the crisis, the slope either flattened or became negative. What is new to this project is the plot of the dashed line, i.e. the percentage of the US population that is connected to Catholic parish, and this is more telling. Until the late 1990s, the percent hovered near 21.5 percent. The proportion increases to a peak of 22.3 percent right after the abuse crisis, then falls 1.2 percentage points through 2007, and hovers at this new level throughout the rest of the timeline. While this time series is not part of the causal evidence that the crisis caused this substantial drop in the Catholic population, the figure informs descriptively what happened to the broad participation of the faithful during this time.

Comparing high allegation to low allegation dioceses, Figure 1.6 displays the plot of event study for the log of the total Catholic population in Panel A and provides us with this visual evidence. Prior to news of the scandal, the coefficients are statistically indistinguishable from zero. The estimates drop beginning in 2002 and become statistically significant 3 years after news of the scandal broke. The effect continues to get larger until the estimates plateau beginning around 2009.

The estimates for the difference-in-differences regression are presented in the first two columns of Table 1.3. Similar to the sacramental outcomes discussed above, the outcome is the log of the Catholic population in a diocese. Both estimates are negative and statistically significant. With controls added, the estimate -0.0741 is statistically significant at the 95 percent level. According to this specification, the 2002 abuse crisis causes about 7.1 percent

decrease in the Catholic population which is nearly 50,000 for the average high allegation dioceses.

These results qualitatively hold if we use the per capita measure, i.e the percent of the total diocesan population that is Catholic. These corresponding estimates are in the third and fourth columns of Table 1.3. With controls added, there is an estimated 1.61 percentage point drop in Catholic percentage in the treated dioceses, corresponding to about 38,000 fewer people connected a Catholic parish in the average affected diocese.

1.4.3 Catholic Parishes

Next, the chapter looks at the structural backbone of Catholicism. Most people experience their faith through their local parish church. It is through this unit that Catholics attend weekly services, participate in service organizations, socialize with fellow churchgoers, and engage their spiritual leaders. As mentioned in the background section, a territorial diocese is divided up into parishes with their own geographical boundaries so that the local church and meet the pastoral and spiritual needs of its members. The growth in new parishes is largely determined by the number of Catholics in the area.

With the reduction of individuals who formally associate with a Catholic parish, what happens to the parishes? We might expect that the number of individual parish churches to decrease for a number of reasons. First, as peo-

ple leave the faith, there is a reduction in demand.[14] Second, the scandal led to many lawsuits. This coupled with fewer members led financial troubles for dioceses. In order to cut budgets, parishes would close or be consolidated.

Panel B of Figure 1.6 offers the event study plot for parish churches. The plot provides no evidence of differing pre-trends, and a decline is apparent after news of the scandal broke. While the estimate of the year coefficient does not become significant until a few years after the scandal, this is in line with the timeline of closing of parishes.[15]

The estimates of the causal impact of the scandal on church parishes are presented in the fifth and sixth columns of Table 1.3. The chapter finds that parish closures increase so the number of churches declines about 7.8 percent, or about 12.7 churches for the average high allegation diocese.

1.5 Results: Schools and Student Enrollment

1.5.1 Diocesan Schools

Among diocesan schools, I first look to the primary schools. The typical Catholic elementary school is parochial, i.e. it is attached to the a parish. In most cases, the school is on the same grounds as the parish church. A

[14] One might posit that it is the closing of parishes that may lead to people leaving the faith. I argue that this is not the main mechanism at work. When a parish is closed or consolidates, the people that were attached to the that parish are automatically assumed into another parish church.

[15] Several dioceses began consultation in the early 2000s about closing and merging certain churches due to population decline, lack of finances, and the shortages of priests. The decrees to actually close the parishes only were promulgated after enough time had passed for discernment and for the affected communities to adapt.

regular practice at these schools is to have the parish priest(s) involved by, perhaps, teaching a weekly religion class, visiting class rooms, celebrating the school liturgy, and/or attending other school functions. It seems reasonable that news of a clergy scandal within the diocese would cause parents to avoid sending their children to these schools and decrease their demand for Catholic education.

In Figure 1.7, the coefficient plots for the log number of diocesan elementary schools and log student enrollment are presented. Both panels give positive visual evidence that there are parallel pre-trends. Immediately when the scandal breaks, declines manifest for both the number of schools and student enrollment. The estimates of the causal impact on diocesan elementary schools are presented in Panel A of Table 1.4. With controls included, the effect on the number of diocesan elementary schools and student enrollment are decreases of about 13.2 percent and 13.2 percent, respectively. The average high allegation diocese experiences declines of about 9.5 schools and 2,900 students.

The chapter finds that diocesan secondary schools in high allegation dioceses also experienced negative effects due to the 2002 abuse scandal. The plots of the event studies for the log number of diocesan high schools and log student enrollment are presented in Figure 1.8. Panel A makes it evident that there is a rather quick decline in the number of high schools in the affected dioceses. As seen in Panel B, the effect on log enrollment takes awhile longer to manifest, around four years after it does for the log number of high

schools. Since a major pipeline of diocesan high school students comes from the parochial school system, it is reasonable that this could simply represent the drop in elementary school enrollment that occurred in the immediate years following 2002. Panel B of Table 1.4 provides the estimates of the effect of the scandal on the number of diocesan high schools and enrollment when controls are included. A high allegation dioceses sees a negative effect on high schools and enrollment of 15.2 percent, or 1.1 schools, and 17.1 percent, or 680 students. Together with the estimated effect on enrollment for elementary schools, this amounts to 180,000 total students, or about 8.0 percent of those enrolled in all diocesan schools in 2001.

As a robustness exercise, I move analysis of the effect on school enrollment from the OCD data set to the constructed one from the PSUS of the NCES. Table 1.5 presents estimates of the effect of the scandal on elementary and high school enrollment using the biennial NCES data. In particular, we examine the log enrollment of first grade because this would mark the beginning of an average student's stint at a parochial school. With similar reasoning for high schools, I measure the effect on the log of ninth grade enrollment.

The first column presents the estimates from the regression using a modified Equation 1.1 with county level controls included. In Panel A, I find that diocesan elementary schools in high allegation dioceses saw about a 6.7 percent decline in first grade enrollment. In the bottom panel, the estimate suggests that diocesan high schools experienced a 9.4 percent drop in ninth grade enrollment. While the magnitudes of these estimates are smaller than

those presented from the diocesan level data, they are in the same direction and tell similar stories. The number of diocesan schools and school enrollment dropped due to the scandal.

1.5.2 Other Catholic Private Schools

As discussed above, one could expect that this scandal might affect Catholic private schools differently than diocesan schools since the former are not under direct control of their respective local diocese and get their identity from their sponsoring organization. The first sign that this may be the case is when I do not differentiate between diocesan and private schools in the analysis. Table 1.6 provides the estimates of the effect on all Catholic schools.[16] The results for elementary schools are qualitatively similar to those of diocesan elementary schools shown in Table 1.4. What differs are the estimates for high schools in Panel B. In fact, the estimated effect on all Catholic high schools is less than half than that on diocesan high schools.

In order to get at the effect on private schools, I use the school level data from the NCES. The second column of Table 1.5 is dedicated to non-diocesan catholic schools. In Panel A, the effect is positive for elementary schools with an estimate of 0.7 percent on first grade enrollment. The estimate is opposite sign of the one for diocesan schools although this estimate is not statistically significant. The corresponding estimate for secondary schools in panel tells

[16] Additionally, Table A.7 provides estimates on the log of these school variables utilizing data from the NCEA. Both this and Table 1.6 tell a similar story that the effects are attenuated for high schools when both diocesan and private Catholic school data are combined.

that the average high allegation diocese experienced a negative effect of 3.2 percent on ninth grade enrollment, about one third of the one measured for diocesan schools. The estimate again is not statistically significant. However, with this caveat, the chapter finds that in high allegation dioceses Catholic private schools fare relatively better than their diocesan counterparts.

1.5.3 Other Religious and Nonsectarian Schools

What naturally follows is to investigate whether the scandal has an impact on other private schools. I consider both other religiously affiliated schools and those that have no proper affiliation and considered secular. An effect seems plausible if parents of students that attend Catholic schools in high allegation dioceses want their children to continue their education at a private institution not affiliated with the sex abuse scandal.

Column 3 of Table 1.5 presents the estimates for the non-Catholic religiously affiliated schools. In Panel A, the negative effect on first grade enrollment is almost 5.4 percent and statistically significant. Both diocesan and religiously affiliated schools suffered declines in elementary enrollment. The effect on religiously affiliated schools is about 80 percent of the diocesan estimate. For nonsectarian schools, the estimate in the fourth column is not statistically significant and translates to about a 0.1 percent decrease in first grade enrollment. Then Panel B of Table 1.5 provides further evidence that the scandal affected non-Catholic private schools differently. Although the estimate is not statistically significant, secular schools experienced a 0.8 per-

cent increase in ninth grade enrollment. Religiously affiliated and nonsectarian schools respectively have 5.9 and 10.4 percentage point differences in the effect on ninth grade enrollment when compared to their diocesan counterparts.

Figure 1.9 provides a plot of the estimates of the regressions from a modified Equation 1.1 on the different log grade level enrollments for diocesan, private Catholic, other religiously affiliated, and nonsectarian schools. In Panel A, the absolute value of the magnitude of the effect of the scandal on parochial school enrollment is largest at the first grade. We visually see that the effect weakens and goes to zero as we look at the later years in parochial school. A natural point for changing schools in the Catholic school system occurs in the ninth grade after a student graduates from a parochial school. The plots show that for each grade level of high school, diocesan school suffered declines in enrollment.

In Panel B, the point estimates are positive but not statistically significant for private Catholic elementary schools. For other religiously affiliated schools, the pattern of the estimates in Panel C for the early grade levels follows a similar pattern to that of diocesan schools. However, there appears to be no effect on grade level enrollment for eighth grade and above. Interestingly, in Panel D, for nonsectarian schools, there is little to no effect of the scandal on grade level enrollment for grades that are at the parochial level. Yet, there is visual evidence of a positive effect and a clear difference from that on diocesan schools for grades at the secondary school level. Overall, the scandal had strong negative effects on diocesan Catholic schools, and there is mixed

evidence of the effect on non-diocesan Catholic and other private schools.

1.6 Conclusion

This study has explored how the faith crisis resulting from the 2002 abuse scandal affected Catholics' attachment to the religion and subsequently other human capital outcomes, like education. Exploiting the variation of the exposure of the scandal at the diocesan level, I use a difference-in-differences specification to estimate the causal impact of the scandal on religious participation. First, there generally is a decline in the demand for religion as people disconnect from their local parish and fewer parents initiate their children in the faith. There is a 7.1 percent drop in the Catholic population. Second, while, in general, Catholic schools hurt, the demand for diocesan controlled elementary and high schools experiences a steeper decline. About 180,000 students leave diocesan schools, and these departures represent nearly 60 percent of the decline in enrollment in all private schools since 2000.

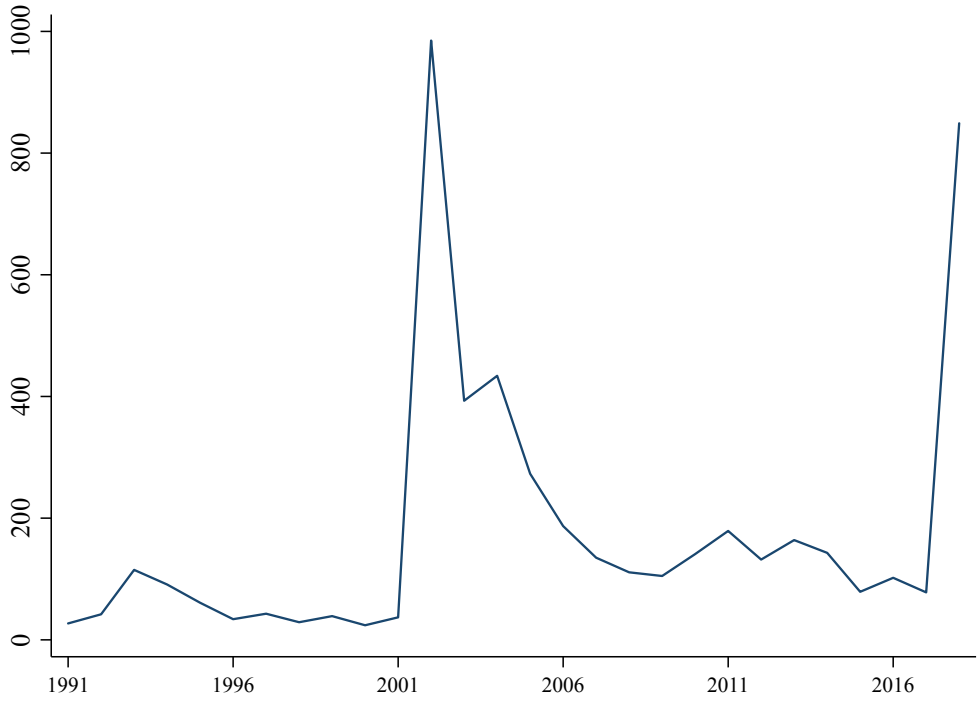
One of the key results is that the demand for religious participation drops, and this potentially informs us of the potential effects of the current turmoil of the abuse scandal within the Church. Due to the recent revelation of past allegations in the 2018 Pennsylvania grand jury report coupled with new allegations against key leaders, many have expressed their displeasure with the governance of the Church. In response to this pressure, many dioceses began releasing lists of priests that have had credible accusations leveled against them. However, it was not a universal practice, and there are still a number

of them that have not released any such list. This chapter has argued that the lagged effects of the 2002 scandal were primarily due to one's hesitance to change one's beliefs until a sufficient accumulation of allegations was reached. Without full transparency, partially disclosing the names of previously accused but publicly unknown clerics might show a similar pattern in the decline in the demand for religion.[17]

More significantly, the results of this chapter demonstrate that a person's faith experience has ramifications for broader human capital outcomes. During this crisis of faith, when religious attachment falls, there is a corresponding drop in the number of parents choosing to educate their children in the diocesan school system. This decrease in demand for Catholic education should lead one to consider what effects this will have on human capital accumulation. Catholic schools historically have educated those in need and have a sound reputation in providing a solid education across all groups of students. Forgoing Catholic education might have long term effects on a person's labor outcomes. The starkness of these outcomes leads one to wonder whether there other aspects of a person's life were affected by the scandal. The next chapter delves into this question investigating the consequences to long term health.

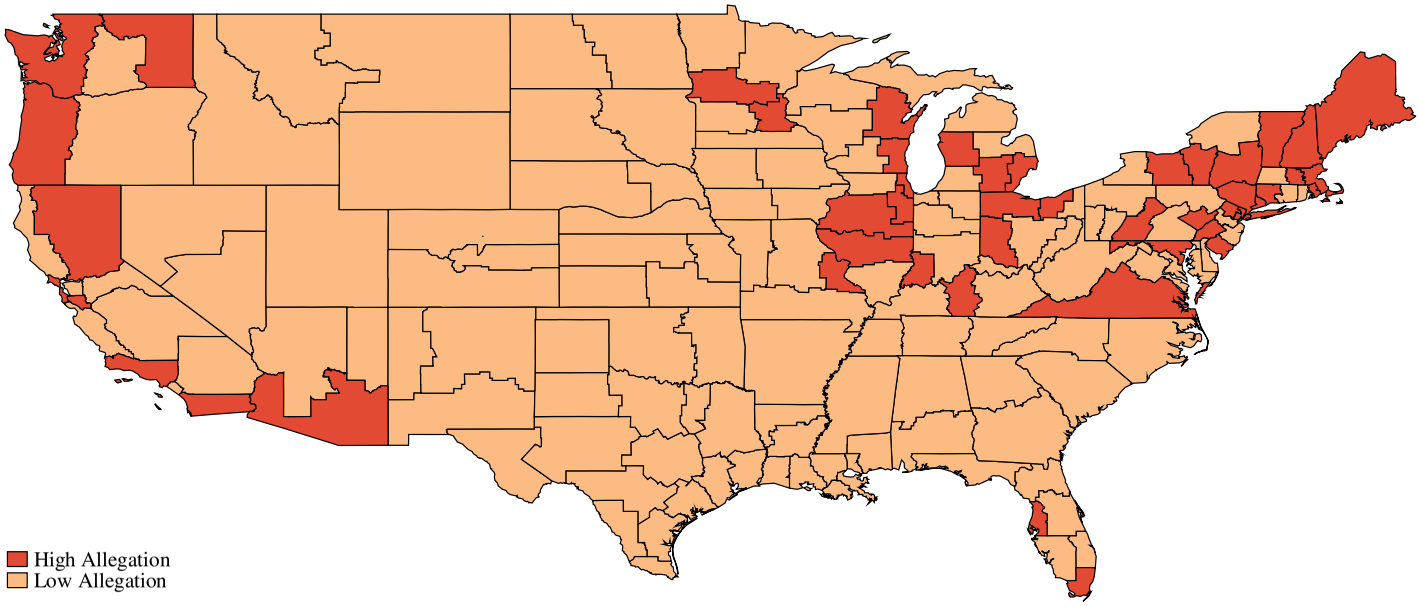
[17]This chapter clearly studied the effect of negative publicity resulting from public revelation of an accused cleric. Entangled with this is that with each newly accused cleric, there was a growing awareness that the local diocese had not been fully transparent about their knowledge of past allegations.

Figure 1.1: First-time Allegations in US: Catholic Clergy and Religious



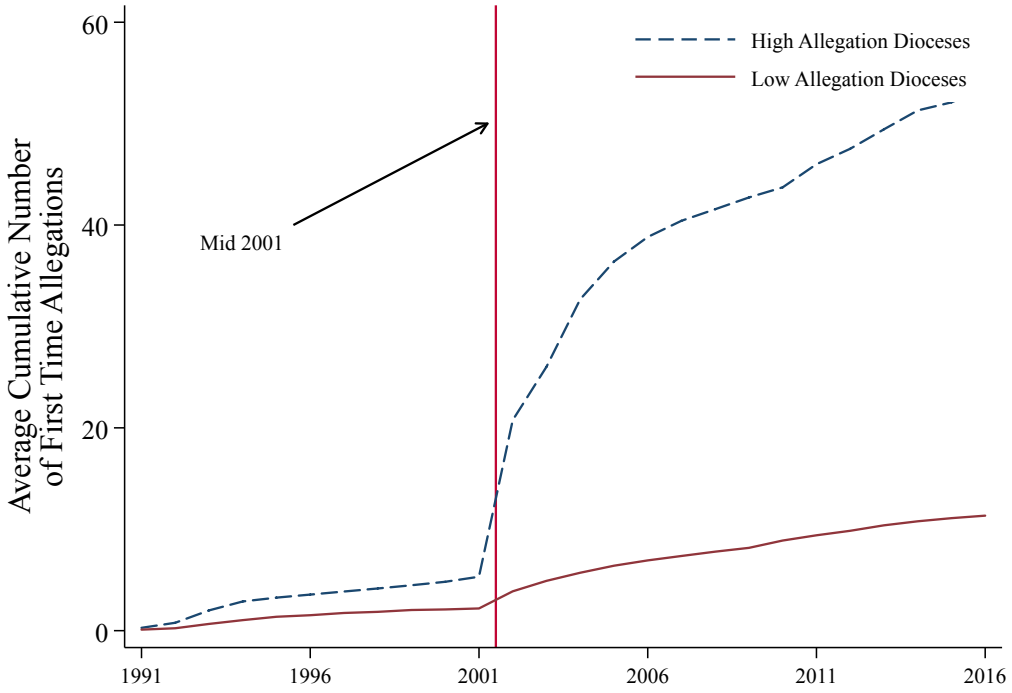
Note: This figure plots the total number of first-time allegations of Catholic bishops, priests, brothers, nuns, and seminarians by year for 1991 to 2018. The first spike occurs in 2002 due to the wave of people coming forward after the investigative reporting of *The Boston Globe*. Data comes from Bishop Accountability.

Figure 1.2: US Roman Catholic Dioceses by Treatment Status



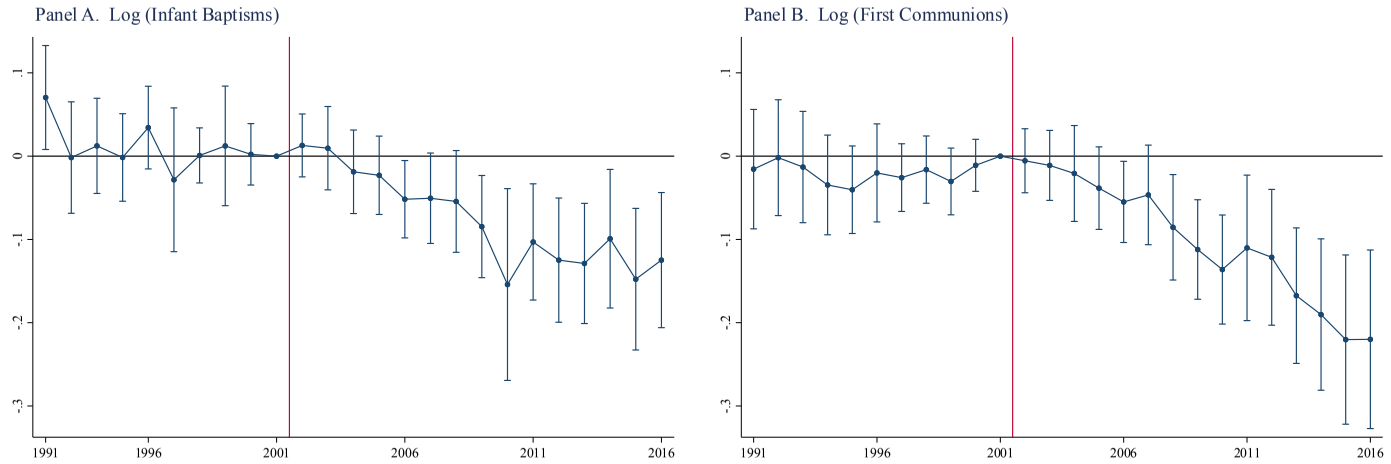
Note: This figure outlines the geographical boundaries of the 171 territorial Latin Rite dioceses in the continental US along with its high or low allegation status based on the number of first-time allegations in 2002. The territorial US Latin Rite dioceses not shown include the Alaskan dioceses (Anchorage, Fairbanks, and Juneau) and the Hawaiian diocese (Honolulu), and they are considered low allegation dioceses.

Figure 1.3: Cumulative First-Time Allegations in US by High and Low Allegation Dioceses



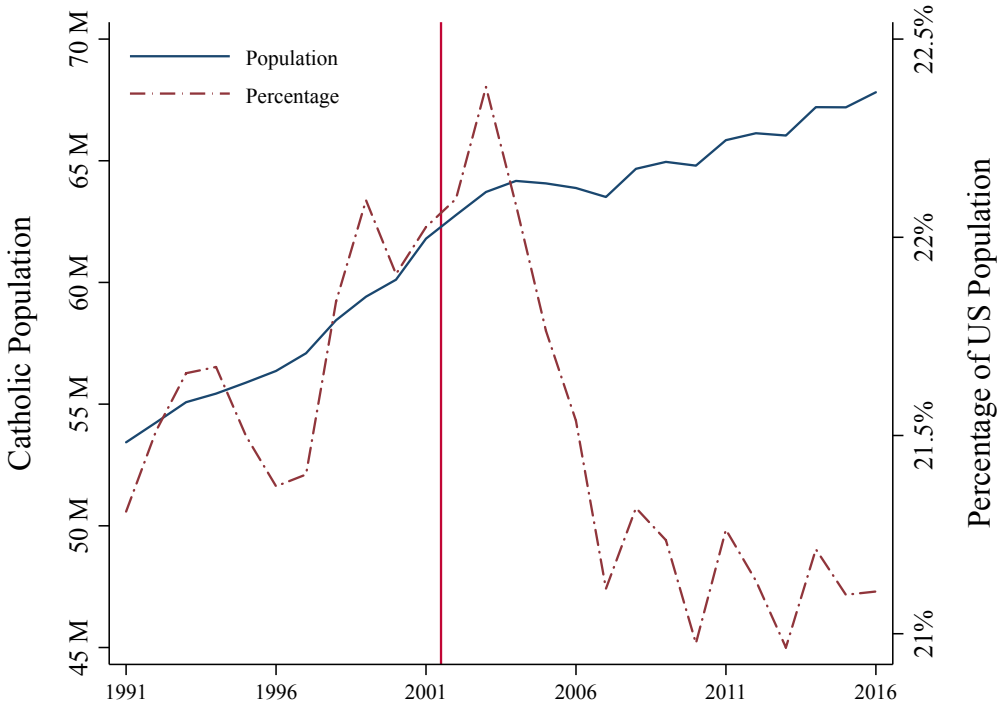
Note: This figure shows the average cumulative number of first-time allegations of Catholic bishops, priests, brothers, nuns, and seminarians by year and high allegation (dashed line) and low allegation (solid line) dioceses beginning in 1991 to 2016. The solid vertical line is immediately before 2002 when news of the abuse scandal first broke nationally. Data comes from Bishop Accountability.

Figure 1.4: Effect on Infant Baptisms and First Communions in the US



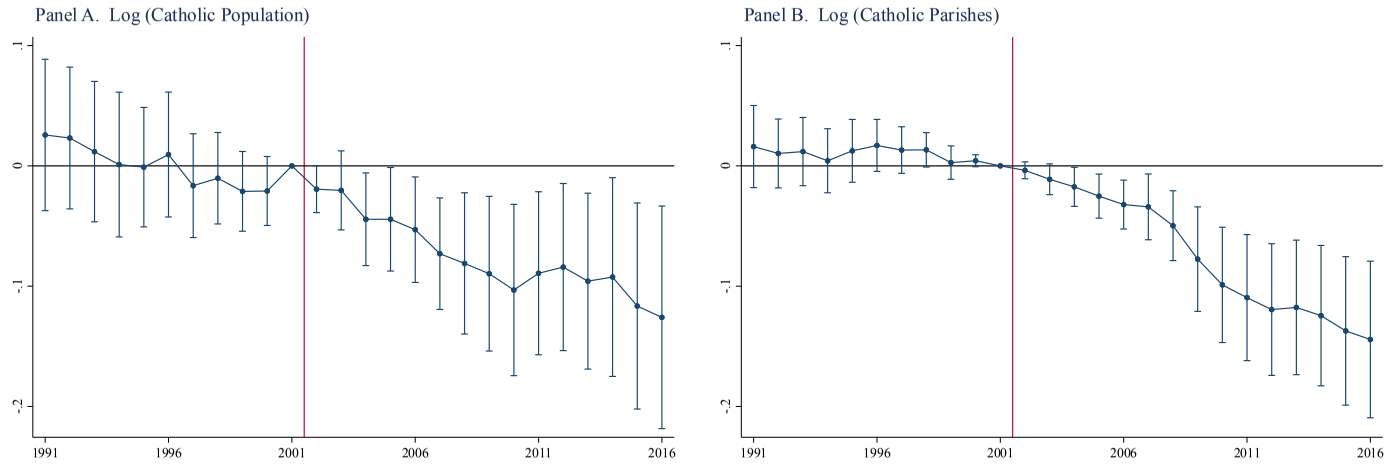
Note: The left panel provides the plots of the yearly coefficients from equation 1.2 for the log number of infant baptisms. The right panel presents the coefficient plot for the log number of First Communions. In each panel, the solid vertical line is immediately before 2002 when the abuse scandal first broke nationally. The capped vertical bars show 95% confidence intervals from standard errors clustered at the diocesan level. These plots provide evidence that the assumption of parallel pre-trends holds and that there is a difference in how the two types of dioceses evolved after the scandal.

Figure 1.5: US Catholics: Population and Percentage



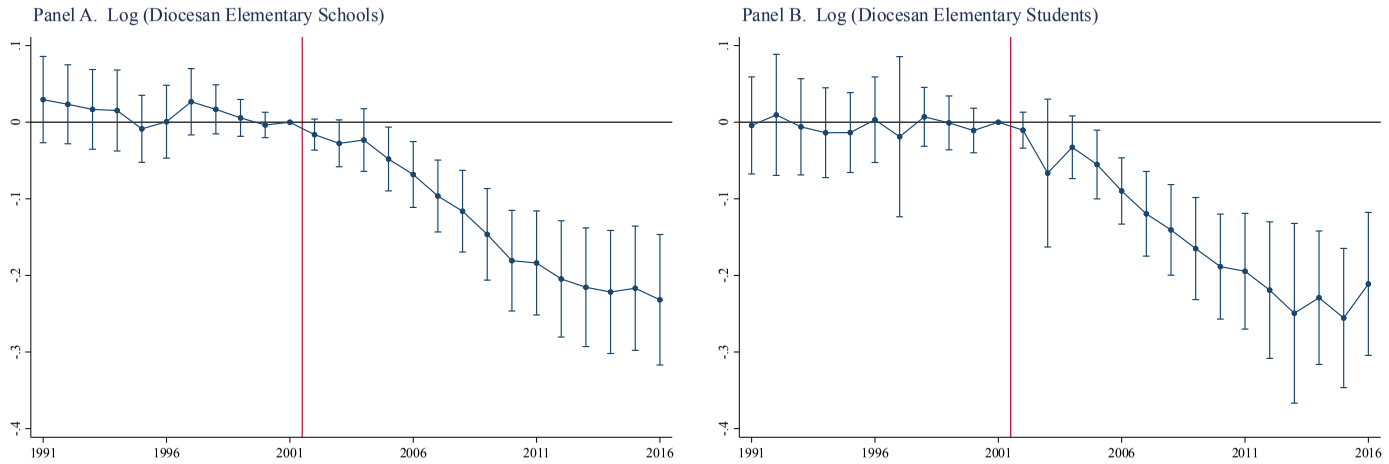
Note: This figure shows key metrics of the US Catholic population from 1980-2017 for the 176 geographical Latin Rite dioceses. The solid line plots the US Catholic population (left axis). The dot-dash line plots the percent (right axis) of the US population that is Catholic. The solid vertical line is immediately before 2002 when the abuse scandal first broke nationally. Data comes from the OCD. While the number of Catholics generally increases throughout the time period, the percentage drops immediately after the 2002 abuse scandal. It hovers near the 21 percent mark from 2006 onward.

Figure 1.6: Effect on Catholic Population and Parish Churches in the US



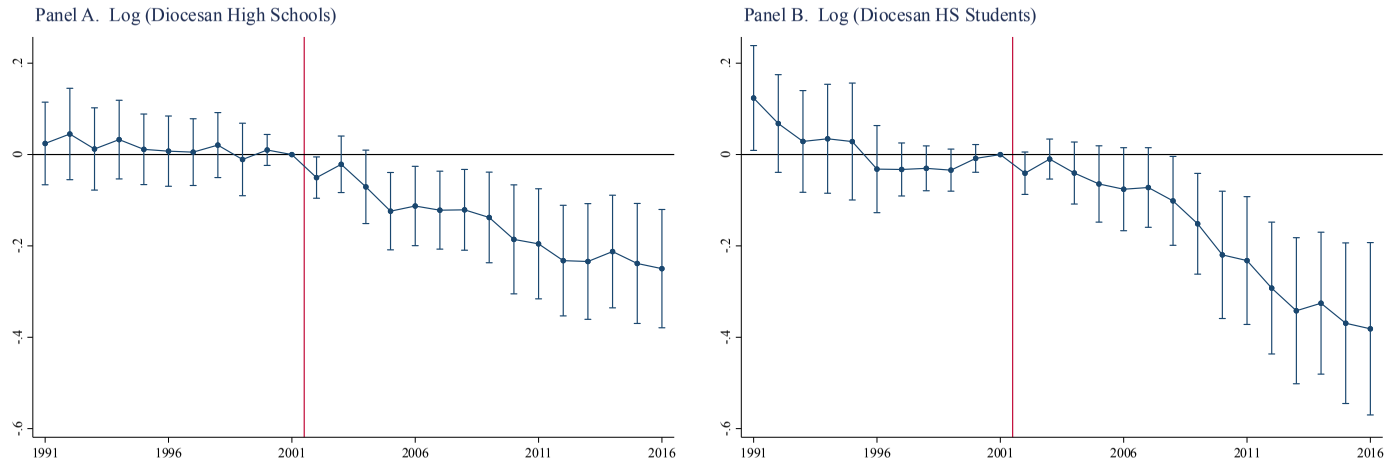
Note: The left panel provides the plots of the yearly coefficients from equation 1.2 for the log number of total Catholics. The right panel presents the coefficient plot for the log number of Catholic parishes. In each panel, the solid vertical line is immediately before 2002 when the abuse scandal first broke nationally. The capped vertical bars show 95% confidence intervals from standard errors clustered at the diocesan level. These plots provide evidence that the assumption of parallel pre-trends holds and that there is a difference in how the two types of dioceses evolved after the scandal. See Figure A.7 for the plots of the event studies when controlled for a linear pre-trend.

Figure 1.7: Effect on Diocesan Elementary Schools and Students in the US



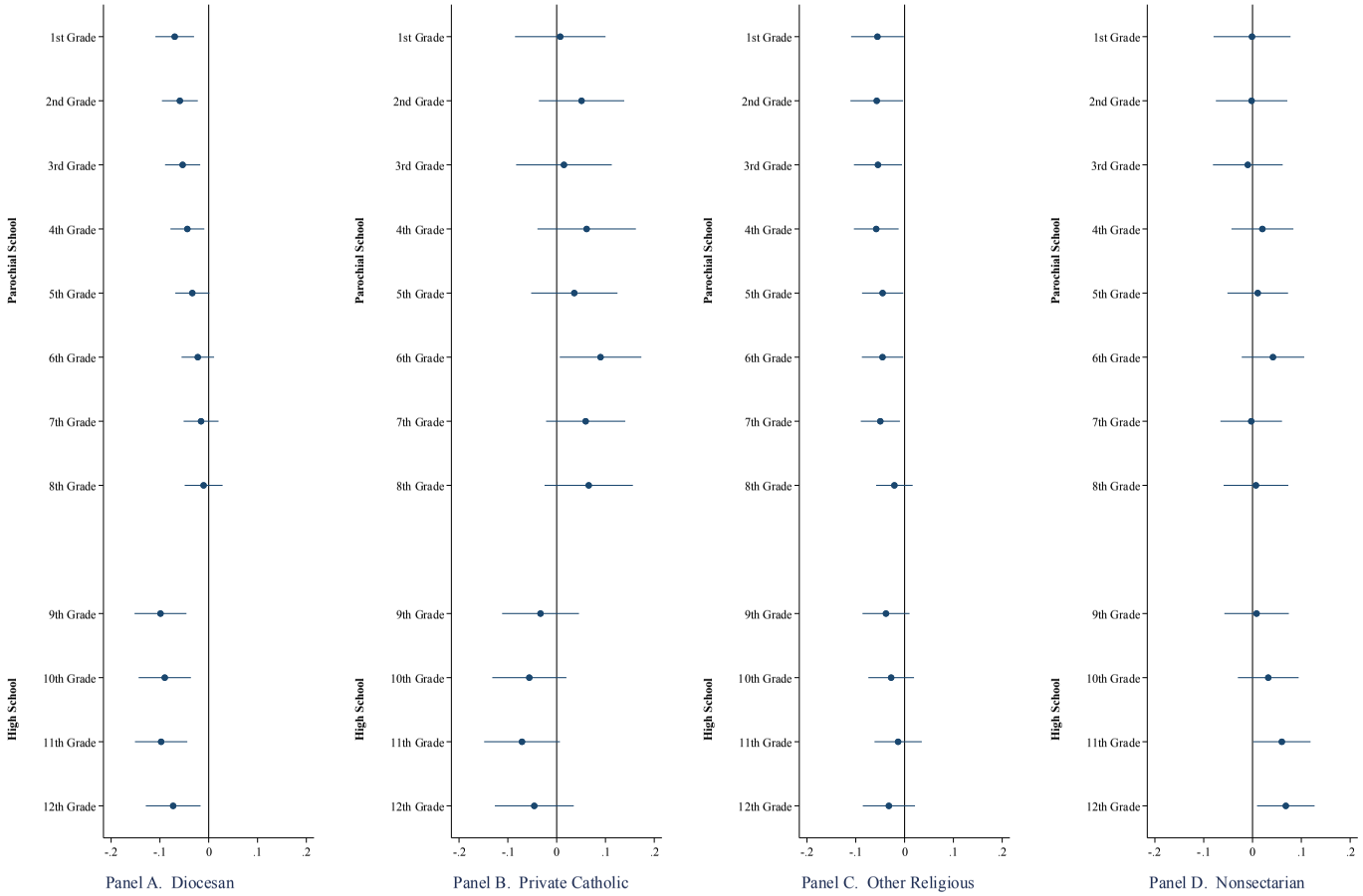
Note: The left panel provides the plots of the yearly coefficients from equation 1.2 for the log number of elementary schools under diocesan control. The right panel presents the coefficient plot for the log total enrollment at diocesan elementary schools. In each panel, the solid vertical line is immediately before 2002 when the abuse scandal first broke nationally. The capped vertical bars show 95% confidence intervals from standard errors clustered at the diocesan level. These plots provide evidence that the assumption of parallel pre-trends holds and that there is a difference in how the two types of dioceses evolved after the scandal.

Figure 1.8: Effect on Diocesan High Schools and Student Enrollment in the US



Note: The left panel provides the plots of the yearly coefficients from equation 1.2 for the log number of high schools under diocesan control. The right panel presents the coefficient plot for the log total enrollment at diocesan high schools. In each panel, the solid vertical line is immediately before 2002 when the abuse scandal first broke nationally. The capped vertical bars show 95% confidence intervals from standard errors clustered at the diocesan level. These plots provide evidence that the assumption of parallel pre-trends holds and that there is a difference in how the two types of dioceses evolved after the scandal. See Figure A.8 for the plots of the event studies when controlled for a linear pre-trend.

Figure 1.9: Effect on Grade Level Enrollment Across Types of Schools



Note: The figure presents the coefficients for the effect of the scandal on enrollment grade level. The horizontal bars show 95% confidence intervals from standard errors clustered at the diocesan level. From left to right, the panels graphically show estimates for diocesan schools, private Catholic schools, other religiously affiliated schools, and nonsectarian, i.e. non religiously affiliated, private schools. In each panel to the left of the y-axis, the grade levels are grouped together according to the usual diocesan school level. Children in first through eighth grade attend parochial schools while grades nine through twelve comprise high school. This breakdown provides a clearer way to see the effect of the scandal on the grade enrollment on the two different school levels. Data comes from the biennial PSUS from the NCES for the years 1991-2016.

Table 1.1: Averages of Key Church Variables by High and Low Allegation Dioceses in 2001

	High Allegation	**Low Allegation**
No. of Dioceses	50	126
No. of Allegations	0.50	0.11
No. of Baptisms	10,665	3,433
No. of First Communions	8,945	3,181
No. of Confirmations	6,335	2,251
No. of Elementary Schools	71.6	23.6
No. of Elementary Students	22,050	6,327
No. of High Schools	7.02	3.44
No. of HS Students	4,001	1,533
No. of Parish Churches	162.7	82.4
Catholic Population	713,630	212,644
Total Population	2,374,070	1,286,634

Note: The table shows the means of key participation variables in 2001, one year before the abuse scandal. The school and enrollment statistics are for Catholic diocesan schools. All high allegation dioceses had observations for the listed outcomes with the exception of the Boston and St. Louis for First Communion. Fourteen low allegation dioceses did not have a high school and one did not have an elementary school. Allegation data comes from Bishop Accountability. All other data is from the OCD.

Table 1.2: Effect of High Allegations on Sacraments of Initiation

	Log Baptism		**Log Communion**		**Log Confirmation**	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	-0.1696***	-0.0821**	-0.1128***	-0.0812**	-0.0323	-0.0023
	(0.0388)	(0.0318)	(0.0427)	(0.0338)	(0.0526)	(0.0465)
Mean Dep. Var.	*10,665*		*8,945*		*6,335*	
Controls		X		X		X
No. of obs.	4,380	4,380	4,547	4,547	4,556	4,556

Note: Diocesan and year fixed effects are included. Standard errors are clustered at the diocesan level. Diocesan controls for all regressions include percentage of diocesan population that is Hispanic, per capita income, and, for Baptism, the percentage of the population that is infant aged. Pre-scandal (2001) means of the dependant variables from high allegation dioceses are in italics. *** p <0.01, ** p <0.05, * p <0.1

Table 1.3: Effect of High Allegations on Catholic Population and Parish Churches

	Log Catholic Pop		**Percent Catholic**		**Log Parishes**	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	-0.0980***	-0.0741**	-1.862***	-1.613**	-0.0823***	-0.0813***
	(0.0374)	(0.0325)	(0.7127)	(0.6730)	(0.0202)	(0.0195)
Mean Dep. Var.	*713,630*		*28.29*		*162.7*	
Controls		X		X		X
No. of obs.	4,561	4,561	4,560	4,560	4,563	4,563

Note: Diocesan and year fixed effects are included. Standard errors are clustered at the diocesan level. Diocesan controls include percentage of diocesan population that is Hispanic and per capita income. Pre-scandal (2001) means of the dependant variables from high allegation dioceses are in italics. *** p <0.01, ** p <0.05, * p <0.1

Table 1.4: Effect of High Allegations on Diocesan Schools

	Log Number of Schools		**Log Student Enrollment**	
	(1)	(2)	(3)	(4)
A. Elementary Schools				
Treat × Post	-0.1394***	-0.1415***	-0.1421***	-0.1418***
	(0.0327)	(0.0322)	(0.0362)	(0.0361)
Mean				
Dep. Var.	*71.6*		*22,050*	
Controls		X		X
No. of obs.	4,539	4,539	4,541	4,541
B. High Schools				
Treat × Post	-0.1868***	-0.1644***	-0.2047***	-0.1871***
	(0.0444)	(0.0380)	(0.0611)	(0.0564)
Mean				
Dep. Var.	*7.02*		*4,002*	
Controls		X		X
No. of obs.	4,108	4,108	4,109	4,109

Note: Diocesan and year fixed effects are included. Standard errors are clustered at the diocesan level. Diocesan controls include percentage of diocesan population that is Hispanic and per capita income. Pre-scandal (2001) means of the dependant variables from high allegation dioceses are in italics. *** p <0.01, ** p <0.05, * p <0.1

Table 1.5: Effect of High Allegations on Private School Enrollment

	Diocesan	**Catholic Private**	**Other Religious**	**Nonsectarian**
	(1)	(2)	(3)	(4)
A. Log First Grade Enrollment				
Treat × Post	-0.0697***	0.0074	-0.0556**	-0.0010
	(0.0201)	(0.0470)	(0.0274)	(0.0400)
Mean				
Dep. Var.	*34.20*	*27.62*	*18.00*	*18.04*
No. of obs.	227,260	227,260	227,260	227,260
B. Log Ninth Grade Enrollment				
Treat × Post	-0.0988***	-0.0330	-0.0381	0.0084
	(0.0240)	(0.0399)	(0.0244)	(0.0335)
Mean				
Dep. Var.	*147.15*	*143.67*	*22.18*	*29.42*
No. of obs.	94,891	94,891	94,891	94,891

Note: Data comes from the biennial PSUS from the NCES for the years 1990-2016. School and year fixed effects are included. Standard errors are clustered at the diocesan level. County controls include percentage of population that is Hispanic, percentage of population that is school aged, and per capita income. Pre-scandal (2001) means of the dependant variables from high allegation dioceses are in italics. *** p <0.01, ** p <0.05, * p <0.1

Table 1.6: Effect of High Allegations on Catholic Schools

	Log Number of Schools		**Log Student Enrollment**	
	(1)	(2)	(3)	(4)
A. Elementary Schools				
Treat × Post	-0.1237***	-0.1272***	-0.1305***	-0.1306***
	(0.0325)	(0.0321)	(0.0358)	(0.0358)
Mean				
Dep. Var.	*74.7*		*22,870*	
Controls		X		X
No. of obs.	4,561	4,561	4,562	4,562
B. High Schools				
Treat × Post	-0.0737**	-0.0615**	-0.0432	-0.0360
	(0.0298)	(0.0380)	(0.0288)	(0.0342)
Mean				
Dep. Var.	*13.74*		*7,988*	
Controls		X		X
No. of obs.	4,307	4,307	4,306	4,306

Note: Diocesan and year fixed effects are included. Standard errors are clustered at the diocesan level. Diocesan controls include percentage of diocesan population that is Hispanic and per capita income. Pre-scandal (2001) means of the dependant variables from high allegation dioceses are in italics. *** p <0.01, ** p <0.05, * p <0.1

Chapter 2

Beyond the Pews: The Impact of Abuse Allegations on Health

2.1 Introduction

We learned from the study in Chapter 1, the sex abuse scandal in the Catholic Church led to a reduction in religious participation and possibly human capital as parents moved their children from diocesan schools. These findings are not surprising as one could easily imagine people's negative reaction to the institution that was responsible for such scandal. The effects of the scandal could easily go beyond these two outcomes because a person's religious identity goes beyond certain religious tenets. Faith can form the whole person, shape how one thinks, and partly comprise one's culture.

This chapter expands on the investigation conducted in the previous chapter. I study how the scandal's impact goes beyond religious institutions. In particular, how do abuse allegations affect the faithful's health? I study the effects on how the negative news of the scandal might lead people to risky behaviors through specifically measuring the outcome of fatalities related to despair. As discussed earlier in the dissertation, this crisis of faith moves people to respond in ways that would decrease the demand for religion and affect

human capital outcomes.[1] I now posit that the exploitation of minors and a violation of the people's trust cut off a lifeline where people had social, emotional, and psychological support. In particular, those who availed themselves of a priest's counsel left the faith and no longer had that outlet to help process the challenges in their life. Abandoning this support structure could lead individuals to despair.

The econometric strategy to investigate how this crisis of faith affected the health of of the faithful is very similar to the one in Chapter 1. I exploit the variation in the exposure of the scandal, as measured by the number of public allegations, across dioceses, i.e., church juridical regions, in a difference-in-differences framework. I compare counties that lie in the upper quartile of dioceses with a high number of allegations in 2002 to all other counties. To do this, I obtain allegation information from the nonprofit Bishop Accountability, and I utilize mortality rate data from the CDC Wonder Database.

This chapter finds that with a decline in formal religious connection, the scandal leads to increases in the incidence of higher mortality rates for suicides, overdoes, and alcohol related liver disease for adults aged 45 to 64. For men aged 55 to 64, the 1.8 percentage point effect for suicides represents 9.9 percent of the increase of the number of counties that have a suicide mortality rate greater than 10 per 100,000. Similarly, the 1.2 percentage point increase for accidental overdoses is 6.4 percent of the increase of the number of counties

[1]See Section 1.5.1 for a discussion how the scandal might lead parents to pull their children from religious education and Catholic schools.

with a high overdose mortality rate. Counties in high allegation diocese also witness 2.0 percentage point increase in deaths from alcohol related liver disease. These results are robust to controlling for demographic characteristics, employment rates, and opioid shipments.

This chapter adds to the larger literature on the economics of religion. In particular, the failures of the institution of the Catholic Church led people astray, and I show that this change in the faith experience of Catholics is linked to larger issues affecting human capital outcomes like health. This is in a similar vein to Clingingsmith, Khwaja and Kremer (2009) which shows that the Hajj can be life changing for those who make the pilgrimage. Along the same lines, papers have shown other ways that religious institutions affect not only the demand for religion but also the well-being of people. For instance, religious leaders can influence followers behavior (Bassi and Rasul, 2017), and missionary work has long lasting, i.e., multigenerational, impacts on human capital ((Valencia Caicedo, 2018); (Calvi, Hoehn-Velasco and Mantovanelli, 2020)).

This chapter contributes to an existing literature studying the impacts of the scandal. Studies have shown that the scandal negatively impacted religious giving, religious employment, and Catholic education ((Hungerman, 2013); (Bottan and Perez-Truglia, 2015); (Dills and Hernández-Julián, 2012)). The link between religiosity and state welfare has also been examined (Dills and Hernández-Julián, 2014). This work complements these studies by adding the human capital dimension of health. This project is the first to show that

the scandal pervades beyond the church by affecting mortality rates of behavioral diseases related to despair.

2.2 Background

From my previous findings, a Catholic in a high allegation diocese was more likely to sever one's relationship to their church due to the sex abuse scandal. Evidence has been presented on the negative effect of this crisis on religious participation in Chapter 1. For a discussion on the events that led to the flood of allegations from the investigative reporting of *The Boston Globe* and on the background of the Catholic Church in the United States, refer to Section 1.2. I now transition to look at how the scandal might lead to behavioral changes that may lead to increasing occurrences of certain diseases related to despair.

Literature in psychology has identified a relationship between religion, depression, and suicide. Those who are highly religious are less likely to be depressed and less likely to attempt suicide (Koenig (2012); Van Praag (2009)). With a decline in religiosity, I conjecture that diseases related to despair might increase due to a couple of mechanisms.

One reason is when a Catholic is having personal difficulties, he or she has the option to see a priest or another minister for spiritual conversation or pastoral care.[2] Those who leave the faith and experience such said challenges

[2]Another avenue in which Catholics receive confidential pastoral care is through the sacrament of Reconciliation, commonly referred to as confession.

more than likely no longer avail themselves of that counseling. As a result the disaffected may never get the help that they desire and need. Another rationale is, for many people, faith is more than just a set of personal beliefs; it is an identity. They may question how an institution culturally ingrained in them could betray them by allowing such atrocities. If people are jarred by the scandal, this could lead to some having existential crisis. Finally, when a person leaves a religion, one also leaves his or her local parish community. The friends and acquaintances that had been a support are no longer as present nor as practically accessible to the person. As a result, one may struggle to find other social circles, and this potentially might lead to isolation. In an attempt to measure despair, the chapter investigates the effect of the scandal on mortality rates related accidental overdoses, suicides, and alcoholic liver disease.

2.3 Data and Methodology

2.3.1 Data

The main identification strategy utilizes information gleaned from the non-profit Bishop Accountability. This study makes use of the same data set, as in Chapter 1, that lists each of the accused priest by diocese with the year that an allegation was initially made public. From this, I tally by year and by diocese the number of first-time allegations independent of when the alleged abuse actually occurred. I continue to use more than five first-time allegations in 2002 as the threshold to determine which dioceses are treated.

Using a diocese to county crosswalk, I am then identify counties that lie in high allegation dioceses and also consider them treated.

For the analysis of the effect on mortality rates, I retrieve county level death data from the publicly available compressed mortality data set through the CDC Wonder online database for the years 1991-2014. In particular, I utilize county level data on the number of deaths by year, gender, age group, and cause. I follow Case and Deaton (2017) to categorize underlying causes of death into accidental drug and alcohol overdose, suicide, and liver disease according to the International Classification of Diseases 9^{th} Edition (ICD9) for the years 1991-1998 and the International Classification of Diseases 10^{th} Edition (ICD10) for the years 1999-2014. One of the constraints working with the publicly available county level data is data suppression. If the number of deaths for any specific group is less than ten then the number of deaths is suppressed.[3]

Intercensal demographic county level data, including race and age, comes from the US Census Bureau. Additionally, county level data on personal income comes from the Bureau of Economic Analysis (BEA).

[3]For instance, if the number of heart disease related deaths for women, aged 35 to 44, in Brewster County, Texas in 2005 is less than ten, the CDC suppresses the actual count in the public data.

2.3.2 Empirical Strategy

Since intensity of the sexual abuse scandal varied across different dioceses within the Church, I exploit that variation in a difference-in-differences framework. In order to measure the intensity of variation, I count the number of allegations as mentioned in Chapter 1. I slightly modify my identification strategy to consider counties as high allegation if they lie in a treated diocese. As I have shown that the number of allegations vary across dioceses, this chapter tests whether or not a diocese which had greater exposure to the scandal is related to the mortality rates of deaths of despair.

The estimating equation to estimate the causal impact of the scandal on the outcomes of interest follow:

$$Y_{ct} = \beta_0 + \beta_1 Treat_c \times Post_t + \beta_2 X_{ct} + \eta_c + \nu_t + \varepsilon_{ct} \tag{2.1}$$

Y is the county level outcome for county c in year t, which particulars are discussed in the paragraph below. $Treat_c$, an indicator variable, is 1 if the county c was in a high allegation diocese in 2002. $Post_t$, the other indicator variable, is 1 if the year t is 2002 or later. β_1 is the coefficient of interest that will give the differential effect of having a experienced a higher volume of allegations in the county on the chosen outcome. X_{ct} is a vector of county level specific characteristics, such as Hispanic percentage, per capita income, manufacturing employment, and unemployment rate. η_c is the difference in outcome due to diocesan effect, ν_t represents the common shocks to all dioceses for a specific year, and ε is the error term.

A reasonable choice for the outcome variable Y_{ct} is the county level mortality rate for a particular underlying cause, i.e. the number of deaths per 100,000 people. However, as mentioned the data section, data suppression does not allow for an accurate estimate of the effect on the rate. Instead, I create a new dummy variable, Y_{ct}^{n}, that is one if the mortality rate is greater than or equal to n. In other words, the outcome is whether a county's mortality rate reaches a certain threshold n.[4]

The event study for the various outcomes of interest comes from modifying equation 2.1:

$$Y_{dt} = \beta_0 + \sum_{T \neq 2001} [\beta_{1T} Treat_c \times \mathbb{1}(t = T)] + \beta_2 X_{ct} + \eta_c + \nu_t + \varepsilon_{ct} \tag{2.2}$$

The treatment variable $Treat_c$ is interacted with a set of indicators, $\mathbb{1}(t = T)$, for each calendar year. As such, the yearly coefficients of interest are the β_{1T}s. I normalize $\beta_{1,2001}$ to zero, so all coefficients are interpreted as changes relative to the year before the news of the scandal broke. The rest follows as above.

2.4 Results: Mortality Rates for Deaths of Despair

Following Case and Deaton (2017)'s categorization of behavior-related conditions related to despair, I analyze the effect of high allegations on the subgroups of men and women, aged 45 to 64.[5] Due to the limitations of the

[4] As this cutoff may seem arbitrary, I am in process of getting the restricted data in order to address this truncation problem. Until then, one of the robustness checks varies the rate, n.

[5] I present in full the subgroup of men, aged 55 to 64 because the results are most comprehensive across the different underlying causes of deaths.

public data, I estimate the effect of high allegations on the incidence of a county having a mortality rate greater than 10 deaths per 100,000 people for each subgroup. Table 2.1 has key mortality statistics for counties by whether they are in high allegation and low allegation dioceses for the specific subgroup of men aged 55 to 64. Similar to the summary statistics presented in Table 1.1, one finds that a county that lies in a treated diocese has a larger population on average than one that does not; however, the overall group mortality rate is similar across the two types of counties. Understanding that economic despair may be one of the drivers for these outcomes, I control for migration, income, unemployment and manufacturing employment.

Another concern rises when considering drug overdose mortality rates. One might posit that the timing of the scandal occurs around the beginning of the opioid crisis in the US. In order to see that this chapter's identification strategy differs from the geographic variation of the opioid crisis, I employ data at the 3-digit zip code level on the annual shipment levels of the most popularly prescribed opioids, oxycodone and hydrocodone, from the Automated Reports and Consolidating System of the DEA. Figure 2.1 presents maps noting the quartiles of annual shipment of these opioids. I note the geographical variation of these shipments differs from the scandal variation shown in the map of figure 1.2. In particular, areas of high annual shipments are in the southern states while very few high allegation dioceses are located there.

Figures 2.2, 2.3, and 2.4 display the plots of the coefficients for the event studies for the underlying causes:(1) suicide, (2) accidental drug overdose and

alcohol poisoning, and (3) alcoholic liver disease and cirrhosis. Each plot gives evidence of parallel pre-trends, and there is a clear increase in the effect after news of the abuse scandal broke. With both suicides and accidental overdoses, there is a statistically significant effect within four years of the news breaking of the scandal. There is a longer delay to see the effect in liver disease and cirrhosis. It takes twice as long for a discernible effect. According to the *Merck Manual*, alcoholic liver disease is more likely to occur in individuals who are chronic drinkers for at least eight years. These diseases take a much longer time to develop, and this could explain the delay of seeing the effect in the coefficient plot. These findings generally hold if we increase the threshold of the mortality rate to 20 or 30 deaths for 100,000. Figures 2.5, 2.6, and 2.7 display the event study plots for these larger cutoffs. In each, the shape of the plot remains relatively the same although the effects take a few years longer to be statistically significant.

Column 3 of Table 2.2 presents the estimates of the causal impact that the scandal had on the incidence of a county having a high mortality rate for men, aged 55 to 64.[6] The three different panels provide the estimates for the underlying causes discussed above. Panel A shows that a county in a high allegation diocese had about a 1.2 percentage point increase of having a overdose related mortality rate greater than or equal to 10 per 100,000. This represents about 6.4 percent of the increase in the number of counties that

[6]Results for both male and female, aged 45 to 54 are also located in the first two columns for reference.

reached this threshold during the period of analysis. There are increases of 1.8 and 2.0 percentage points in the incidences of a treated county having a high mortality rate for suicide and liver disease related deaths which respectively represent 9.9 percent and 8.1 percent of the total increase in the number of counties having this mortality rate threshold. The estimated effects on female mortality rates are presented in Columns 2 and 4. The effects are smaller than the estimates for males for each of the causes.[7] To put this in context, I refer to literature on natural disasters. Although there have been mixed results, Kõlves, Kõlves and De Leo (2013) review several non causal studies which correlate natural disasters with suicide rates. There are many that have shown increases. On the upper end of the list, the suicide rate of middle aged men in areas which have experienced earthquakes rises by about 45 percent in Taiwan. While the chapter measures the effect on the incidence of high mortality rate rather than on the actual mortality, the estimated effects fall in line with this literature. This chapter concludes that the scandal leads to an increase in the incidence of high mortality rates related to despair for middle aged men.

[7] A concern is that the counties in which register 10 or more deaths in a single category are larger counties. Table 2.3 provides estimates when observations are restricted to counties who lie in the top half of the total population distribution. Results are qualitatively similar to those of the main specification.

2.5 Conclusion

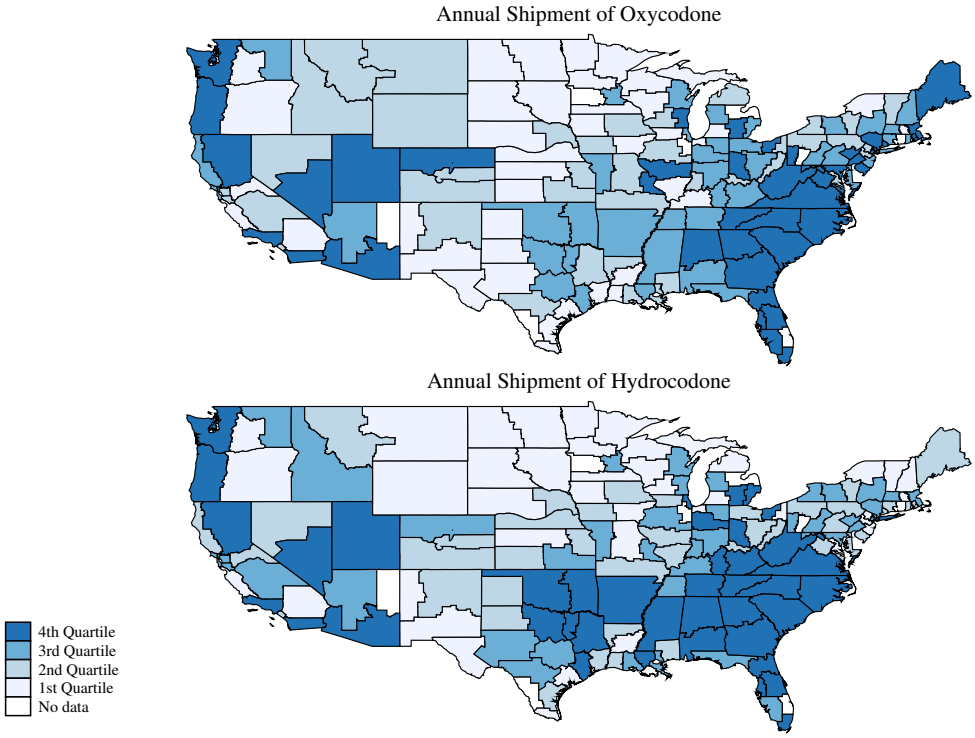
This chapter along with Chapter 1 has explored how the faith crisis resulting from the 2002 abuse scandal affected Catholics' attachment to the religion and subsequently other human capital outcomes, like education and long term health outcome. Exploiting the variation of the exposure of the scandal at the diocesan level, I use a difference-in-differences specification to estimate the causal impact of the scandal on mortality rates.

This study finds that the effects of the scandal extend beyond religious institutions. Counties that had more exposure to scandal had a increased incidence of high mortality rates for accidental overdoses, suicides, and alcohol related liver disease. The scandal explains respectively 6.4 percent, 9.9 percent, and 8.0 percent of the increases in the number of counties that experienced high mortality rates. One of this chapter's limitations is that it can only measure the incidence of high mortality rates due to data censoring. This motivates future study with restricted data from the CDC to do more comprehensive analysis.

Yet, these consequences to long term health imply that detachment from faith increases the likelihood of deaths related behavioral related diseases associated with despair. As such, these findings argue that the effects of shocks to one's faith experience are not limited to a church building or even to the larger religious institution. Additional future research should consider how religion reaches beyond its own sphere and to take into account people's faith experience when studying topics related to well-being, including other human

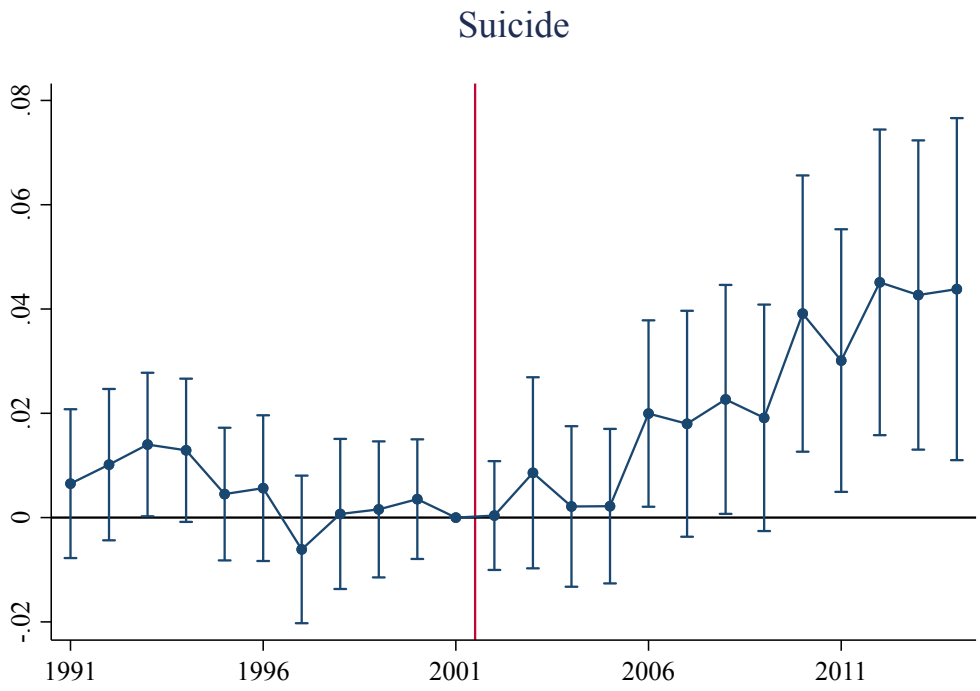
capital outcomes.

Figure 2.1: US Roman Catholic Dioceses by Annual Shipment Levels of Select Opioids in 2002



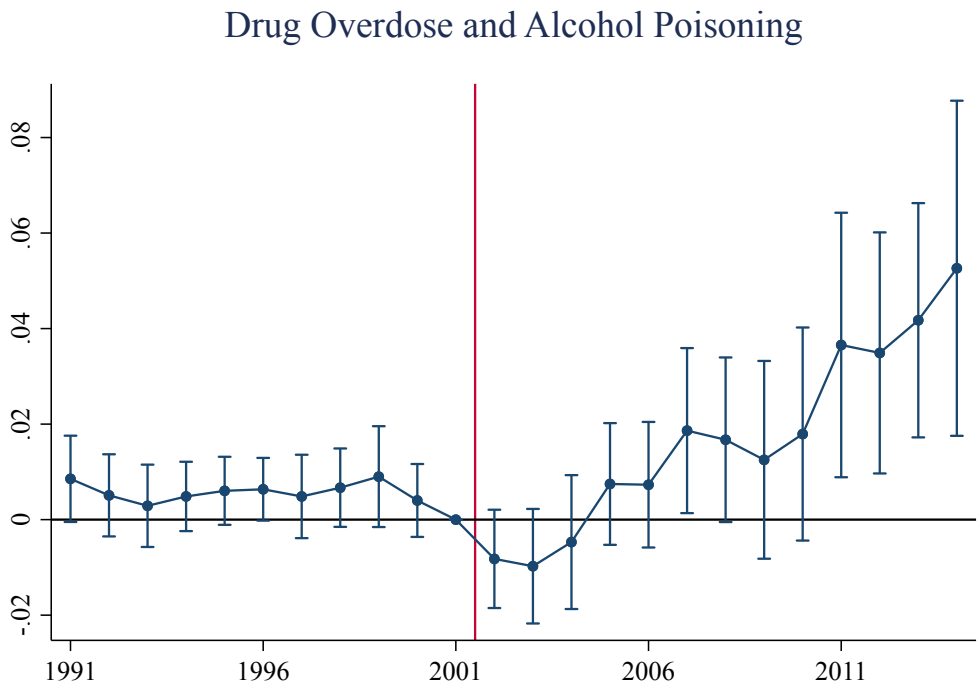
Note: The top panel presents the annual shipment level of oxycodone as reported to the DEA in 2002, and the bottom maps is of the annual shipment level of hydrocodone by US dioceses. It is clear to note that the geographical variation of the shipment level of these controlled substances differ from the paper's identification of high and low allegation dioceses in Figure 1.2. Data is from the ARCOS at the DEA for the year 2002.

Figure 2.2: Effect of High Allegations on Incidence of Mortality Rate Greater than 10 per 100,000 for Men, Aged 55 to 64 (Suicide)



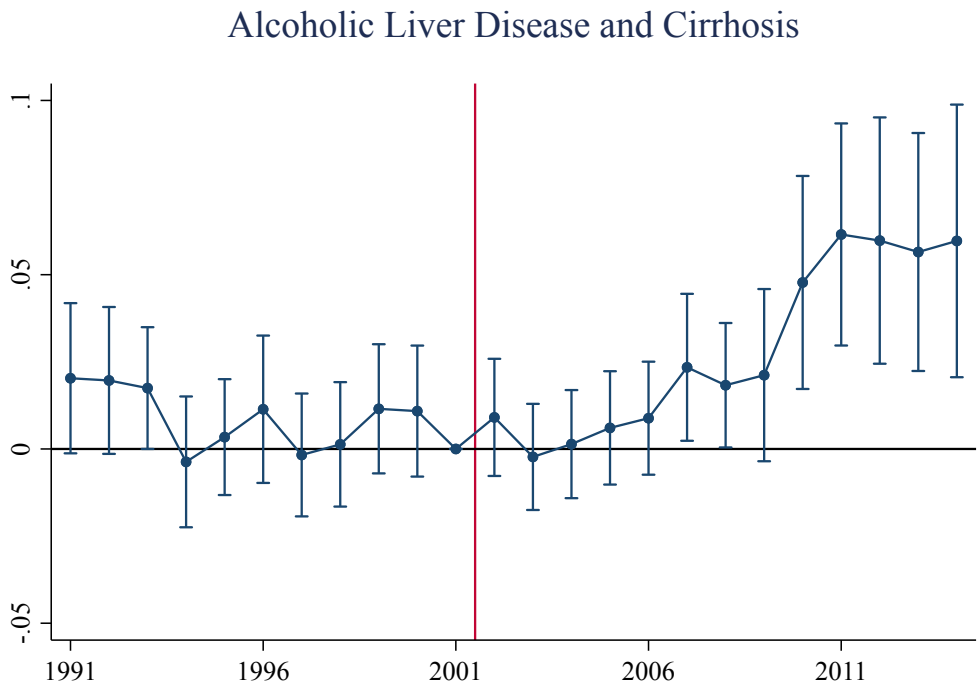
Note: This is the plot of the yearly coefficients for whether a county's mortality rate for deaths from suicide is greater than 10 per 100,000 from the analogous county level specification of equation 1.2. The solid vertical line is immediately before 2002 when the abuse scandal first broke nationally. The capped vertical bars show 95% confidence intervals from standard errors clustered at the diocesan level. These plots provide evidence that the assumption of parallel pre-trends holds and that there is a difference in how the two types of dioceses evolved after the scandal.

Figure 2.3: Effect of High Allegations on Incidence of Mortality Rate Greater than 10 per 100,000 for Men, Aged 55 to 64 (Overdose)



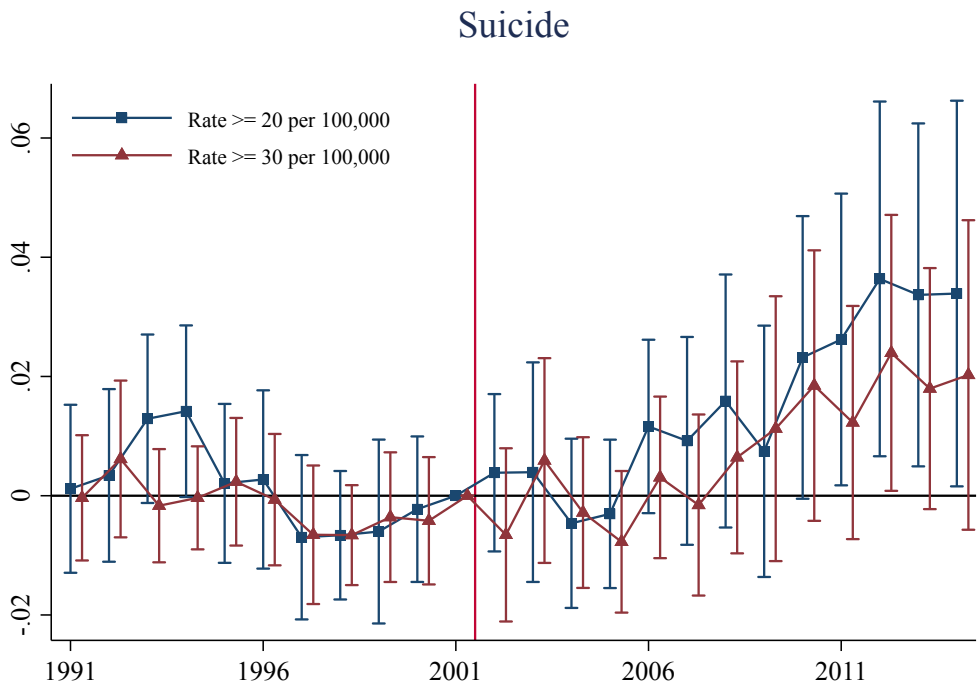
Note: This is the plot of the yearly coefficients for whether a county's mortality rate for deaths related accidental drug overdose and alcohol poisoning is greater than 10 per 100,000 from the analogous county level specification of equation 1.2. The solid vertical line is immediately before 2002 when the abuse scandal first broke nationally. The capped vertical bars show 95% confidence intervals from standard errors clustered at the diocesan level. These plots provide evidence that the assumption of parallel pre-trends holds and that there is a difference in how the two types of dioceses evolved after the scandal.

Figure 2.4: Effect of High Allegations on Incidence of Mortality Rate Greater than 10 per 100,000 for Men, Aged 55 to 64 (Liver)



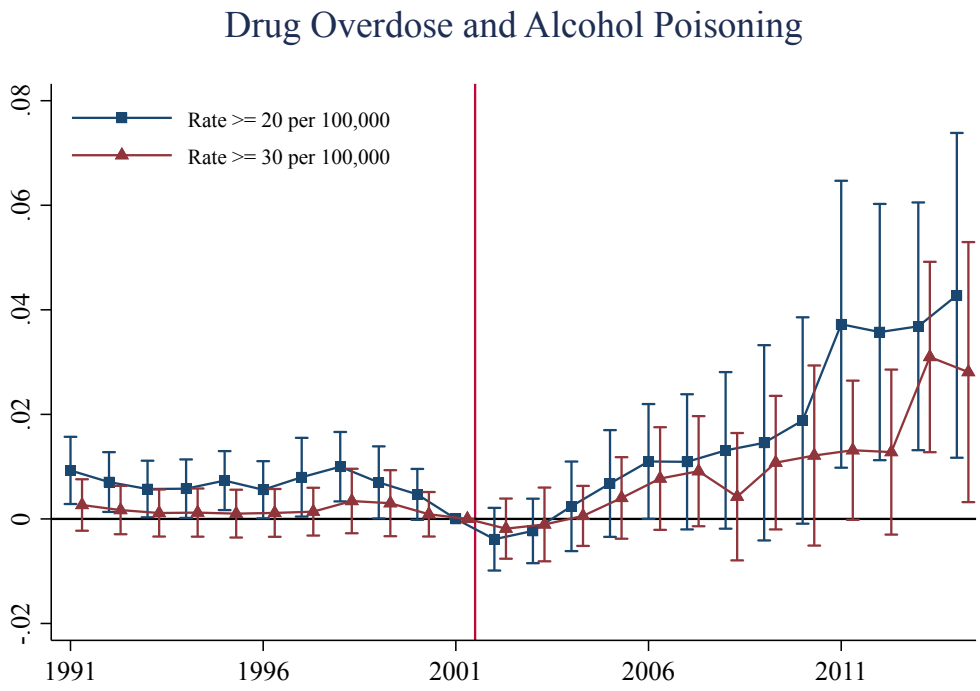
Note: This is the plot of the yearly coefficients for whether a county's mortality rate for deaths related to alcoholic liver disease and cirrhosis is greater than 10 per 100,000 from the analogous county level specification of equation 1.2. The solid vertical line is immediately before 2002 when the abuse scandal first broke nationally. The capped vertical bars show 95% confidence intervals from standard errors clustered at the diocesan level. These plots provide evidence that the assumption of parallel pre-trends holds and that there is a difference in how the two types of dioceses evolved after the scandal.

Figure 2.5: Effect of High Allegations on the Incidence of Mortality Rate Greater than a Set Threshold for Men, Aged 55 to 64 (Suicide)



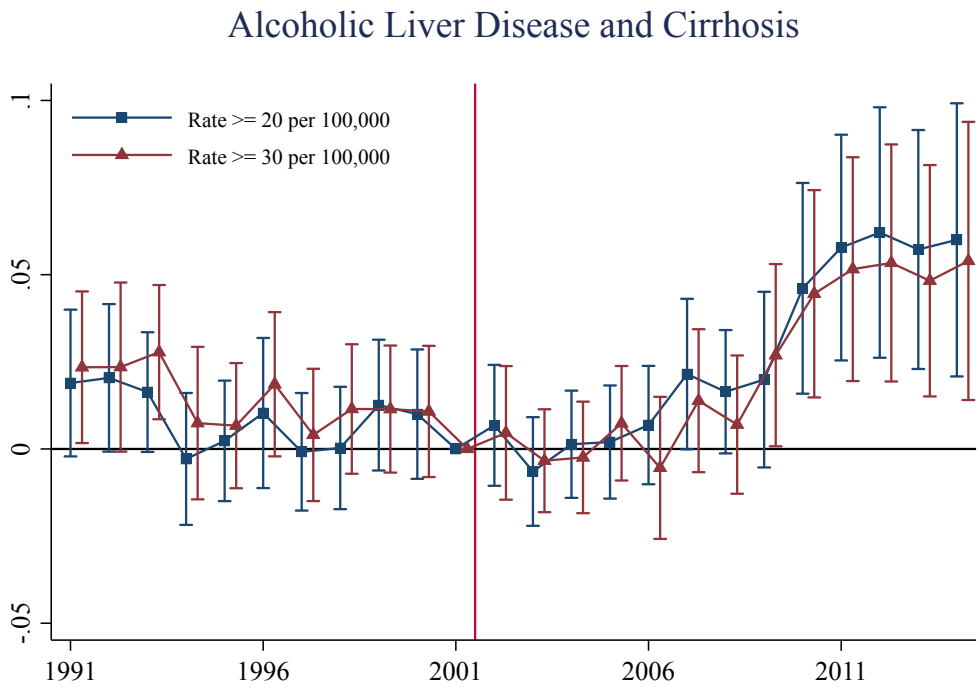
Note: These coefficient plots are for the mortality rate for suicide. The blue square is the coefficient plot for whether a respective underlying cause's mortality rate is greater than or equal to 20 deaths per 100,000. The red triangles are for the threshold of 30 deaths per 100,000. 95% confidence intervals are included. The solid vertical line is immediately before 2002 when the abuse scandal first broke nationally. We see that the general pattern holds from when we used the baseline threshold of 10 deaths per 100,000 as presented in Figure 2.2.

Figure 2.6: Effect of High Allegations on the Incidence of Mortality Rate Greater than a Set Threshold for Men, Aged 55 to 64 (Overdose)



Note: These coefficient plots are for the mortality rate for accidental drug overdose and alcohol poisoning. The blue square is the coefficient plot for whether a respective underlying cause's mortality rate is greater than or equal to 20 deaths per 100,000. The red triangles are for the threshold of 30 deaths per 100,000. 95% confidence intervals are included. The solid vertical line is immediately before 2002 when the abuse scandal first broke nationally. We see that the general pattern holds from when we used the baseline threshold of 10 deaths per 100,000 as presented in Figure 2.3.

Figure 2.7: Effect of High Allegations on the Incidence of Mortality Rate Greater than a Set Threshold for Men, Aged 55 to 64 (Liver)



Note: These coefficient plots are for the mortality rate for alcoholic liver disease and cirrhosis. The blue square is the coefficient plot for whether a respective underlying cause's mortality rate is greater than or equal to 20 deaths per 100,000. The red triangles are for the threshold of 30 deaths per 100,000. 95% confidence intervals are included. The solid vertical line is immediately before 2002 when the abuse scandal first broke nationally. We see that the general pattern holds from when we used the baseline threshold of 10 deaths per 100,000 as presented in Figure 2.4.

Table 2.1: Key County Level Mortality Statistics for Men, aged 55 to 64, by High and Low Allegation Dioceses in 2001

	High Allegation	**Low Allegation**
No. of Counties	549	2,370
Average Group Population	10,364	4,088
Average Total Population	217,335	69,334
Average Group Overall Mortality Rate	1,193	1,429
Number of Counties with Group Mortality Rates $\geq$ 10 per 100,000		
Suicide	17	14
Accidental Overdose	5	0
Liver Related	38	38

Note: The table shows key county level mortality statistics for the demographic group of men, aged 55 to 64, in 2001, one year before the abuse scandal. There is data for 2,919 out of 3,141 counties in the United States. All data comes from the compressed mortality files from the CDC Wonder database.

Table 2.2: Effect of High Allegations on Incidence of Counties with Mortality Rate ≥10 per 100,000

	Aged 45-54		Aged 55-64	
	Male	Female	Male	Female
	(1)	(2)	(3)	(4)
A. Accidental Overdose				
Treat × Post	0.0412***	0.0239**	0.0117*	0.0072
	(0.0138)	(0.0103)	(0.0069)	(0.0045)
No. of obs.	65,524	65,524	65,524	65,524
B. Suicide				
Treat × Post	0.0281***	0.0050*	0.0175**	0.0008
	(0.0081)	(0.0029)	(0.0071)	(0.0018)
No. of obs.	65,524	65,524	65,524	65,524
C. Alcoholic Liver Disease				
Treat × Post	0.0090	0.0023	0.0200**	-0.0005
	(0.0063)	(0.0042)	(0.0082)	(0.0035)
No. of obs.	65,524	65,524	65,524	65,524

Note: The outcome variables are indicators whether a county experienced a mortality rate greater than 10 per 100,000 for each of the individual death categories. Data comes from the CDC Wonder database for the years 1991-2015. County and year fixed effects are included. Standard errors are clustered at the diocesan level. County controls include percentage of population that is Hispanic, per capita income, manufacturing employment, and unemployment rate. *** p <0.01, ** p <0.05, * p <0.1

Table 2.3: Effect of High Allegations on Incidence of Counties with Mortality Rate ≥10 per 100,000 Large Counties

	Aged 45-54		Aged 55-64	
	Male (1)	Female (2)	Male (3)	Female (4)
A. Accidental Overdose				
Treat × Post	0.0407***	0.0258**	0.0121	0.0086
	(0.0151)	(0.0124)	(0.0084)	(0.0057)
No. of obs.	34,192	34,192	34,192	34,192
B. Suicide				
Treat × Post	0.0265**	0.0054	0.0162*	0.0003
	(0.0090)	(0.0038)	(0.0083)	(0.0024)
No. of obs.	34,192	34,192	34,192	34,192
C. Alcoholic Liver Disease				
Treat × Post	0.0058	0.0013	0.0165*	-0.0019
	(0.0081)	(0.0057)	(0.0099)	(0.0049)
No. of obs.	34,192	34,192	34,192	34,192

Note: The outcome variables are indicators whether a county experienced a mortality rate greater than 10 per 100,000 for each of the individual death categories. Data comes from the CDC Wonder database for the years 1991-2015. Only the upper half of counties in population are included in the regressions. County and year fixed effects are included. Standard errors are clustered at the diocesan level. County controls include percentage of population that is Hispanic, per capita income, manufacturing employment, and unemployment rate. *** p <0.01, ** p <0.05, * p <0.1

Chapter 3

The Impact of Noise Pollution on Academic Outcomes

3.1 Introduction

The production of clean energy has expanded considerably in the United States over the past two decades. A major source is wind energy. Beyond providing clean energy to the electric grid, wind farms bring in new jobs and increased tax revenue to rural communities.[1] Yet, recent studies have shown there are hidden costs to wind farms, such as noise.

Noise pollution – defined as any unwanted or disturbing sound that affects health and well-being – is ubiquitous and affects millions of people each day. As early as 1968 and as recent as 2019, noise has been described as a public health crisis. Many studies have found that noise negatively impacts human health and cognition,[2] and there is evidence that this is a channel by which noise can impair educational outcomes of students. We believe it is critical to gain a better understanding of the impact of noise pollution on students' educational outcomes, health, and behavior, and this research will be one of the first to measure the magnitude of these effects.

[1] Douglas (2019) reports that even after installations there will be room for business growth. As technical jobs will be needed to maintain wind turbines, this may lure people to move to areas to provide this skill.

[2] Negative effects of noise on human health and cognition have been widely documented and discussed in popular media (Bosker (2019); Bosker, Hassenfeld and Katz (2019); Owen (2019)). In response to noise exposure, the body releases stress hormones, causing increases in blood pressure and heart rate. Research has linked noise exposure with adverse effects such as stress, fatigue, poor concentration, reduced sleep quality, and productivity loss in the workplace. Repeated exposure to noise has been associated with more serious outcomes such as high blood pressure, heart disease, diabetes, dementia, and depression. While loud noises above 85 dB are of particular concern, low levels of noise also have negative effects on health and cognition. For example, people respond to noises as low as 33 dB, roughly between the volume levels of whispers and suburban areas at night, which can cause sleep disruption and reduce memory reprocessing during sleep.

This chapter studies the impact of noise from wind turbines on student achievement, namely by examining how Texas public school students do on standardized tests. Wind is a rapidly increasing source of renewable electrical energy in the United States, nearly tripling in electrical output from 2010 through 2018,[3] and wind turbine construction has increased in many states over that period. Despite its role as a cost-efficient source of clean energy, wind turbines have been reported to produce low-frequency noise pollution and cause adverse health effects in people living in the vicinity of wind turbines, a phenomenon known as "Wind Turbine Syndrome."[4] The U.S. Department of Energy notes that wind turbines can produce noise from generator hum and blade rotation, and that at distances greater than 400 meters from a wind turbine, these noise levels are "typically" 40 dB and below.[5] For the first time in 2018, the World Health Organization included noise pollution from wind turbines in its *Environmental Noise Guidelines for the European Region*, recommending that policymakers take measures to reduce noise pollution from wind turbines to levels below 45 dB.

Our econometric strategy to investigate how wind turbines affect student performance through this channel is to exploit the variation in the timing of the installation of wind turbines and their locations using a difference-in-

[3]Wind is the second largest source of renewable energy in the United States (behind solar energy), producing 2 percent of total electricity generation and 39 percent of electricity generation from renewable energy in 2018. Electricity generated from wind has nearly tripled from 95 billion kilowatt-hours in 2010 to 275 billion kilowatt-hours in 2018 (U.S. Energy Information Administration (2020)).

[4]Reported symptoms include nausea, sleep disorders, fatigue, and increased stress.

[5]Sounds as low as 33 dB can cause sleep disruption.

differences framework. The wind energy industry has exploded in the last few decades. In 1999, Texas Senate Bill 7 was signed into law that deregulated the electricity market. Tied to this legislation was a mandate to increase clean energy production. As such, since then there have been more than 15,000 wind turbines installed in Texas.

The strategy relies on data from the United States Wind Turbine Database (USWTDB) which has installation date, the latitude, and the longitude for each installed wind turbine in the country. We couple this with publicly available school directory data and standardized test data from the Texas Education Agency (TEA) to geolocate schools and turbines. With this, we compare schools that are near wind turbines to schools that lie further away. While the results qualitatively hold if we increase or decrease the distance, we ultimately decide to consider schools that are 10 or fewer kilometers away from a wind turbine.[6]

Under our preferred specification controlling for school characteristics, we find that wind turbines decrease reading test scores by 4.6 percent of one standard deviation. Interestingly, we find no significant effect on overall student math performance. Additionally, we discover that certain subgroups of students experience the greater brunt of this negative effect. The reading scores of non-minority students drop 8.2 percent of a standard deviation. Additionally, on the reading portion of standardized tests, female students do 2.2

[6]Approximately 5% of the schools in our sample falls within 10 km of a wind turbine. There will be a further discussion about our distance criterion in the results section.

percent of standard deviation better than male students.

This study extends this literature to other environmental factors that can affect human health and productivity. An extensive literature has found that air pollution has negative effects on human health.[7] Other papers have added to show that air pollution is associated with reductions in worker productivity (Graff Zivin and Neidell (2012)). Additionally, we contribute to the literature that looks at factors in the school environment that affects the education production function. Studies have discovered that air pollution leads to decrease performance on standardized tests (Ebenstein, Lavy and Roth (2016); Heissel, Persico and Simon (2019); Persico and Venator (2019)).

There is little work in the economics literature that studies noise pollution, and to our knowledge no prior work has studied the causal effect of noise pollution on educational outcomes. Other literature has offered suggestive evidence that noise can impact student performance. Bronzaft and McCarthy (1975) provide suggestive evidence that noise pollution may reduce cognitive performance and educational outcomes of children.[8] We posit that the nega-

[7]Many papers – such as Chay and Greenstone (2003), Currie and Neidell (2005), and Currie and Walker (2011) – study the impact of air pollution on health by using infant health and mortality as an outcome. This choice of an outcome variable is due to difficulties with measuring adult health; issues include unobservable cumulative lifetime exposure to air pollution, and the possibility of harvesting effects, where those in poor health are most affected by air pollution.

[8]They found, in one New York City public school adjacent to subway tracks, that students on the side of the school facing the tracks achieved lower scores on a reading achievement test compared to students on the side facing away from the tracks. In 1978, after New York City reduced noise from the tracks and installed acoustical installations in the school, the gap in test scores between the two groups of students disappeared.

tive effects on health and cognition from exposure to noise are the channel by which noise pollution impacts educational outcomes.

We add to the growing literature on the effects of wind energy generation. We complement both Zou (2020)–who finds that both sleeplessness and suicides rise due to noise pollution resulting from the installation of wind farms[9]–and Brunner, Hoen and Hyman (2021)–who find the expansion of wind energy increases school district revenue but has no impact on academic performance. We shift focus from health to education, use more granular data to study outcomes on the school level, and also find negative consequences due to noise pollution.

3.2 Public Schools and Wind Energy in Texas

As the wind energy industry rapidly grew in Texas, it was only a matter of time before turbines would be installed near a public school. The public school system is quite vast. More than 5 million students attend one of the nearly 10,000 public schools in Texas. The vast majority of these schools fall under the jurisdiction of one of the 1,013 independent school districts.[10] Each independent school districts have governance in how their students are educated with overall supervision from the statewide Texas Education Agency.

[9]For a detailed discussion on the noise created by wind turbines and the potential health consequences from low frequency noise, see the Background section of Zou (2020)

[10]In this chapter, we do not include charter schools or certain state-run schools in our analysis. We do this in part because these schools can attract students from further distances than the typical public school.

School district boundaries are not necessarily tied to any natural, county, or city borders. For instance, certain districts can overlap cities or counties while others may lie completely within a county. With few exceptions, students attend schools in school districts in which they reside. Each school district also sets attendance boundaries for each of its schools.

Texas has the largest installed wind energy capacity among all states in the United States. In fact the next closest state, Iowa has less than half of the of energy capacity. Figure 3.1 displays the time series of the expansion of wind farms in Texas. The solid line illustrates that the initial capacity began only at 90 MW in 1999, and, over the next two decades, wind energy quickly exploded to nearly 30,000 MW.[11] In the same figure, the dashed line shows the number of wind turbines that were installed annually over the same time period of our study. While there are some years where there were few installations, there are several years of quite heavy expansion.

Panel A of Figure 3.2 plots the location of the 15,524 wind turbines in Texas. We see that the majority of the turbines are located in West Texas to take advantage of natural wind patterns. Wind farms also populate the southern part of the state. Additionally, we highlight a group of 16 counties

[11]This rapid expansion occurred in part to the passage of Texas Senate Bill 7 in 1999 which deregulated the electricity market. Tied to this legislation was a mandate for utilities to invest in renewable energy and increase capacity to 2,000 MW by 2009, doubling the then US wind energy capacity. Wind energy companies leased and purchased land first in West Texas to take advantage of the wind patterns. In the spirit of the legislation, there was little oversight on where wind turbines were installed. For a detailed history of the wind energy expansion in Texas, see Galbraith and Price (2013).

to provide a greater depth of analysis. Panel B offers this zoomed in view of the selection. In this section of the state, there are 326 wind turbines. There are 51 schools that fall within a 30 km radius of a turbine, including 13 that lie 10 km or nearer. Public schools located within 10 km to 30 km of a wind turbine are denoted with blue crosses. Schools that fall quite close to a turbine, i.e. less than 10 km, are marked with a black x. Although many wind turbines are located away from large population centers, there are some schools located nearby them.

3.3 Data and Methodology

3.3.1 Data

Data on installed wind turbines comes from the United States Wind Turbine Database (USWTDB). Information about each wind turbine in the US includes the year it became operational, its capacity in kilowatts, and its latitude and longitude. Additionally, we get school directory information, including site street address, from the Texas Education Agency (TEA) for all public schools in Texas. With the aid of the Texas A&M Geoservices, we obtain the latitude and longitude for each school. Using this information we geocode each wind turbine to every Texas public school that lies within a certain distance. We then create a panel dataset that has annual data on the number of installed turbines with their total energy capacity that falls near a school.

We merge this panel dataset on wind turbine capacity at the school

level with student achievement data. In particular, we use publicly available data from the Texas Education Agency (TEA) for standardized tests scores on the Texas Assessment of Knowledge and Skills (TAKS) and State of Texas Assessments of Academic Readiness (STAAR) exams. We obtain average reading and math scores along with associated demographic data for each public school campus for the school years 2002-2003 to 2018-2019. We then standardize all test scores to mean zero and standard deviation one within subject, grade, year. One of our limitations is that we do not have student level data, and this makes it difficult to test certain aspects of the robustness of our results.[12]

This work focuses on schools that lie within 10 km radius of wind turbine. We chose this for a few primary reasons. First, Zou (2020) studies and find effects on both sleeplessness and suicide rates for individuals who reside within 25 km of a wind farm. Second, according to this same study, noises at low frequency persist longer. For instance, low frequency noise is attenuated by 20 dB relative to the source 20 km away. Zou posits that although low frequency noise may be inaudible, it may still stimulate a biological response. As such, studies should be conducted about the possible consequences from exposure to these sounds. Finally, as we further discuss in the results section, the effects hold when we either contract or expand the threshold of what constitutes a near school. The 10 km radius is in the middle of the range of distances that yield significant results.

[12]Some of these concerns will be discussed in our Empirical Strategy section.

Table 3.1 provides summary statistics on observables across schools that are close to wind turbines to those that are farther away. Column 1 provides information on the public schools that lie within 10 km of a wind turbine prior to the installation of the first turbine. Column 2 gives the characteristics for these schools after the wind turbine has been installed. Column 3 summarizes the same statistics for all other schools. For the sample in our analysis, we focus only on public schools that are regular institutions, i.e. not alternative disciplinary or charter schools. The average z-scores for both reading and math tests for schools near turbines before installation are similar to the scores of schools outside the 10 km radius. We see in the periods after turbine installation, z-scores are lower for close schools; Column 4 relays that the difference is not statistically significant. There is a notable difference in the size of the schools due to the fact the schools near turbines are more likely in rural areas. Demographic characteristics in schools close to turbines are statistically different than those of other schools. In particular, students are more likely to be economically disadvantaged and eligible for the National School Lunch Program. We address this by controlling for these characteristics in the regression analysis. Interestingly, the fraction of students who are at-risk for dropping out from school are similar between the two types of schools. Finally, the average school, within 10 km of a turbine, has nearly 25 turbines nearby.

3.3.2 Empirical Strategy

We will estimate the causal effect of noise pollution on educational outcomes using a difference-in-differences methodology. We posit that there are two plausible channels through which noise pollution can affect students: (1) sleep disruption at their homes as Zou (2020) shows and (2) audible noise during class at school. Our method studies a combination of both channels– disruption that occurs during the day as well as at night.

Under this scheme, our empirical analysis estimates how noise pollution occurring in the vicinity of elementary, middle, and high schools during school hours negatively affects students' educational outcomes, as measured through test scores. Using geolocation data of public schools and sources of noise pollution, we calculate the distance between schools and sources of noise pollution. Specifically, we identify wind turbines that are near schools. As such, we set up the treatment variable for our estimating equation, detailed below, as a binary variable that equals one if the school was within an K-kilometer radius of a source of noise pollution at a given point in time and zero otherwise. We decide on a threshold for what constitutes a school being near a turbine by estimating the treatment effect for different distances.[13] There are a variety of distances for which an effect is detectable, and we use 10 km as a reasonable choice for our preferred specification.

The chapter estimates the causal impact of noise pollution on outcomes

[13]In the results section, we detail that the treatment effect dissipates at 30 kilometers.

of interest using the estimating equation for the preferred specification that follows:

$$Y_{gst} = \beta_K Post_{st} \times Near_{Ks} + X_{st}\delta + \eta_g + \gamma_s + \lambda_t + \varepsilon_{st} \tag{3.1}$$

where Y_{gst} is the z-score of the grade g average test score for school s during year t, β_K is the parameter of interest that estimates the causal effect of noise pollution, $Post_{st}$ indicates periods after a wind turbine installation event, $Near_{Ks}$ indicates whether school s is within K kilometers of a noise pollution source, X_{st} is a vector of time-varying school-level covariates, η_g is a grade level fixed effect, γ_s is a school fixed effect, λ_t is a year fixed effect, and ε_{st} is an error term. School-level controls include the fractions of students who are male, white, Hispanic, economically disadvantaged, and at-risk for dropping out.

The event study for the various outcomes of interest comes from modifying equation 3.1:

$$\begin{aligned} Y_{gst} = &\sum_{T \neq t_{-1}} [\beta_T Near_{Ks} \times \mathbb{1}(t_h = T)] + X_{st}\delta + \eta_g + \gamma_s + \lambda_t + \varepsilon_{st}, \\ &\text{for } h = -5, 4, -3, \cdots, 3, 4 \end{aligned} \tag{3.2}$$

The treatment variable $Near_{Ks}$ is interacted with a set of indicators, $\mathbb{1}(t_h = T)$, for each calendar year. As such, the yearly coefficients of interest are the β_Ts. t_0 represents the year of the wind turbine installation. t_{-1} is omitted, and I normalize $\beta_{t_{-1}}$ to zero. As such, all β_T coefficients are interpreted as changes relative to the year before installation. The rest follows as above.

The estimates that we derive from our specification are causal under the following assumption: in the absence of a source of noise pollution, educational outcomes for students attending schools within K kilometers of a noise source would have followed the same pre-trend as that of schools that were outside an K-kilometer radius of a noise sources. Our analysis does yield that both treated and untreated schools have parallel pre-trends so this assumption seems credible.

There are possible limitations to our identification strategy. First, it is possible that the introduction of wind turbines would reduce the region's reliance on energy from fossil fuels, which produce air pollution. For instance, in the areas of new wind farm installations, traditional energy sources, such as coal plants, may cut back their capacity and/or production. To the extent that the introduction of wind turbines is associated with a reduction in air pollution, our results underestimate the estimate of the impact of wind turbines on educational outcomes.

Second, the installation of wind turbines can lead to compositional changes in the student body of nearby schools. As it seems unlikely that the installation of wind turbines is unanticipated, families may decide to move away once they hear the news, possibly because of concerns about the impact on their home's value or negative health effects. Maybe the introduction of green energy is displacing workers in the fossil fuel (or other) industries. Perhaps families with at least one parent working in green energy are moving in (and maybe these people are better educated, more wealthy, and/or have

children with greater human capital development); or perhaps people are only moving out, and the only families that stay in the treated areas are the poorest and/or least educated. The direction of this bias is ambiguous. Unfortunately, with the public data, we cannot directly address this last concern but we will tackle this in a future study with data from the Texas Education Research Center when we have access to administrative individual level data.

3.4 Academic Achievement Response to Wind Turbines

3.4.1 Distance from Wind Turbines

Our main specification relies on determining a threshold for what constitutes a school being close to to a turbine. In order to come up with plausible values for the *Near* variable in Equation 3.1, we estimate using several different values for this treatment variable. A reasonable distance according to the previous literature hovers around 25 km. As such, we vary the threshold for the school's distance from a turbine from 5 km to 30 km. Figure 3.3 plots the pooled estimates for the effect on reading test scores. Its shows that the estimates are negative and significant at the smallest distances. At 20 km, the estimated effect is still significant at the 10 percent level. The pooled effect nears zero for all schools located with 25 km of a turbine. The estimates for math scores in Figure 3.4 provide a similar story in how the estimates dissipate.

For the next part of analysis, we bin groups of schools by how far they are from wind turbines. In Table 3.2, the six columns each provide the estimate

for different 5 km-sized bins of nearby schools. In Panel A, the estimates by bin for reading test scores show unsurprisingly the strongest effect is for the nearest schools. For the 132 schools that fall in the 0-5 km bin, there is nearly a 11 percent of a standard deviation decrease in test scores. The estimates continue to be negative for the bins up to 20 km, but they are not significant. Finally, the estimates for the furthest bin is close to zero. The estimates of the effect on math scores in Panel B follow a similar pattern with estimate of the final grouping at 25-30 km a noisy zero. These findings make it reasonable to assume that any effects most likely dissipate around 30 km. After previewing these results, we find that using a base threshold of 10 km seems acceptable going forward.

3.4.2 Main Results

Figure 3.5 provides the event study plot derived from Equation 3.2 on the effect of wind turbines on student reading and math performance on state standardized tests. The difference in how the outcomes evolve by the two groups of schools, i.e. (1) those near wind turbines and (2) all others, is evident in this coefficient plot. In the five years prior to the installation of the turbine, the coefficients for the reading z-score regression are close to zero and not statistically significant. It seems plausible that the assumption of parallel pre-trends for reading performance holds. In the year of installation, there is a significant drop of 0.091 standard deviations in scores. This continues in the following years as the estimates remain negative and statistically significant

through the first full three years that a school is located within 10 km of a wind turbine. By the fourth year, the effect curiously tapers off and loses statistical significance.

Why might this trend occur? Perhaps, the effect dissipates after a couple of years because schools make efforts to abate the noise by installing sound proofing. Additionally, it may also happen due to families in their home life also taking mitigating action. Or, simply, students may learn how to adapt due to the increased noise. To further investigate this in the future, we will strive to acquire anecdotal data on whether schools near turbines took any action to deal with any perceived increases in noise. We also will use individual level data to see if and how the measured effect changes during a student's academic career.

Figure 3.6 provides the event study plot of the effect of wind turbines on student math scores. It is clear and evident that there is a downward linear trend prior to the installation of a wind turbine. Because of the pre-trend we are cautious in the way we interpret the effect. This chapter seems to have better a experimental design for studying the effect on reading performance than math performance. Due to this pre-trend, there is a bias to find an negative effect on the standardized math scores.

The primary results are presented Table 3.3. Panel A reports the estimates of the effect of wind turbines on reading test scores. Column 1 shows that relative to the years before a wind turbine is installed, scores on the reading portion of standardized tests drop by a significant 0.046 standard de-

viations. We include more stringent controls in Column 2 by interacting the grade fixed effects and the school fixed effects. This yields a quite similar, statistically significant effect of 0.043 standard deviations. The estimates of the effect on math performance are in Panel B. The point estimate in Column 1 implies that there is a 2.3 percent of one standard deviation decline in math scores. Although the estimated effect on math scores has a smaller magnitude than the one one on reading, the difference between the two is not statistically significant. As such, we cannot dismiss the notion that wind turbines have a similar effect on reading and math performance.

We next investigate how wind turbines affect various subgroups of students. Figure 3.7, Figure 3.8, and Figure 3.9 respectively plot the event studies for (1) male and female students, (2) white, black, and Hispanic students, and (3) gifted and at-risk students. Remarkably, each subgroup has evidence of parallel pre-trends. Looking at the effect by ethnicity in Figure 3.8, the estimated effect on white students (Panel B) is approximately an 8 percent of one standard deviation decline in test scores while the one on black students (Panel C) is smaller and not significant. In fact, there is not a clear effect on black students. The estimates of the effect for Hispanic students (Panel D) follow the general pattern to those on all students. The effect tapers off by the fourth year. Interestingly, the coefficients for the effect on white students stay negative, grow in absolute value, and remain statistically significant throughout the event horizon. While the event studies for most subgroups investigated have a similar shape, the estimated effects on white students appears the most

persistent.[14]

We discover there are heterogeneous effects when we look at the pooled estimates for the disaggregated results. In Table 3.4, Column 1 has the estimate of the preferred specification for all students. Column 2 presents the effect on males which is similar to the one for all students. In Panel A, males experience 5.6 percent of a standard deviation decline in their reading scores. The estimate for female students in Column 3 is smaller in magnitude and not significant. Since this exercise is an example of multiple hypothesis testing, we generate Benjamini, Krieger and Yekutieli (2006) sharpened q-values as described in Anderson (2008) and provide them in angle brackets in the table. With these revisions, both estimates are significant. Additionally, we test whether the difference on whether the estimated effects on reading scores is significant in in Column 4. The difference of 2.2 percent is, in fact, significant. In Panel B, while all the estimates on math scores are negative, neither the estimates nor the difference between them are statistically significant.

Table 3.5 provides the pooled estimates for white, black, and Hispanic students. The statistically significant estimate on reading scores for whites, in Column 2, is quite large at -0.082. This estimate remains statistically significant even with the sharpened q-value. While both black and Hispanic students experience a smaller effect on reading scores than white students, only the difference between black and white students is statistically significant.

[14]The event study plots for the effect on math performance are included in Appendix B.

White students on average score 0.062 standard deviations lower than black students after the installation of a wind turbine. In Panel B, the estimates for the effect on math scores reveal that white students experience a significant -0.089 effect. Additionally, Hispanic students do 12 percent of a standard deviation better than their white counterparts in the post period.

Finally, we examine the differences between gifted students and at-risk students in Table 3.6. Compared to the overall effect, gifted students experience a stronger effect of -0.074 on reading scores. When corrected for multiple hypothesis testing, the estimate's sharpened q-value falls just outside the 10% significance level. While not significant, the point estimate of the effect for at-risk students is slightly positive. Yet, the rather large difference between the estimated effects of the two groups of students is -0.102 and statistically significant. In Panel B, the effects on math scores are positive but not significant. Overall, after looking at the different groups, we find that the effect of wind turbines on academic outcomes weighs more heavily on three: male students, white students, and gifted students.

3.5 Conclusion

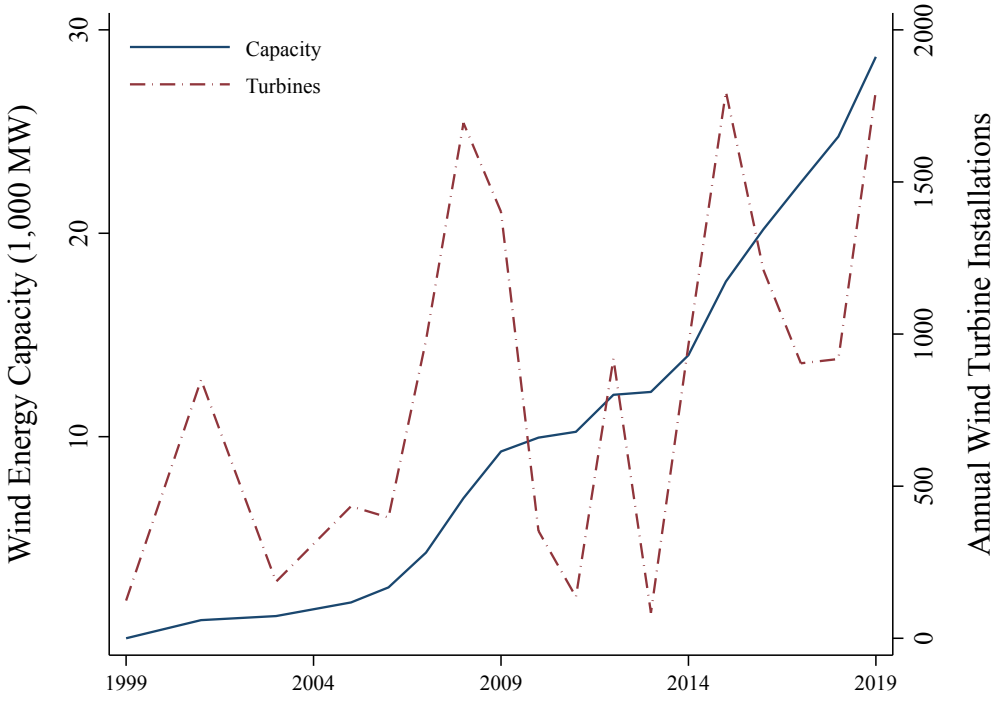
This chapter presents a quasi-experimental analysis of the impact of noise pollution from wind turbines on educational outcomes of students attending elementary schools in Texas. Using aggregated standardized test scores at the school level and data on geolocations of schools and wind turbines, we use a difference-in-differences design to study how the introduction of wind tur-

bines impacted reading and mathematics scores of students attending nearby schools. While there is no overt counter evidence to the parallel pre-trend assumption for reading scores, there is potential flaw in our experimental design in investigating math scores. There is a linear pre-trend that positively biases us to find an effect on math scores.

We find that wind turbines are associated with a 0.046 standard deviation decline in reading scores, but no statistically meaningful change in math scores. Additionally there are heterogeneous effects among certain subgroups of students. Reading scores for males are 0.022 standard deviations lower than those of females. Non-minorities also appear to face the brunt of the effect as the reading scores for white students drop by 0.082 standard deviations.

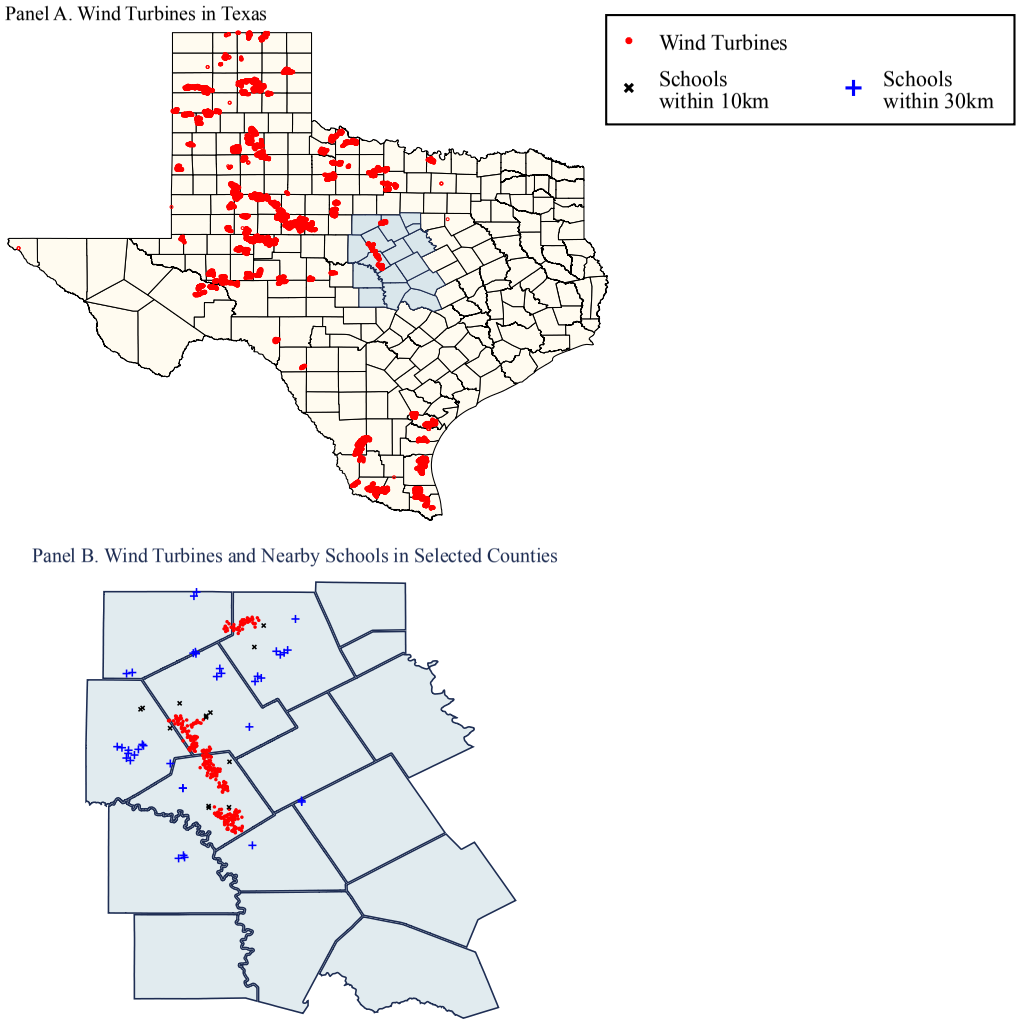
We interpret these results as providing evidence of a hidden cost of installing wind turbines in rural communities: the disruption of human capital accumulation of children. Yet, we understand the limitations to this current study. We believe that an improved research design using detailed, individual-level data on test scores, migration, and health outcomes presents an opportunity for future work that will allow us to explain the mechanisms behind this chapter's results and more credibly study the impact of noise on student outcomes.

Figure 3.1: Texas Wind Energy and Turbines



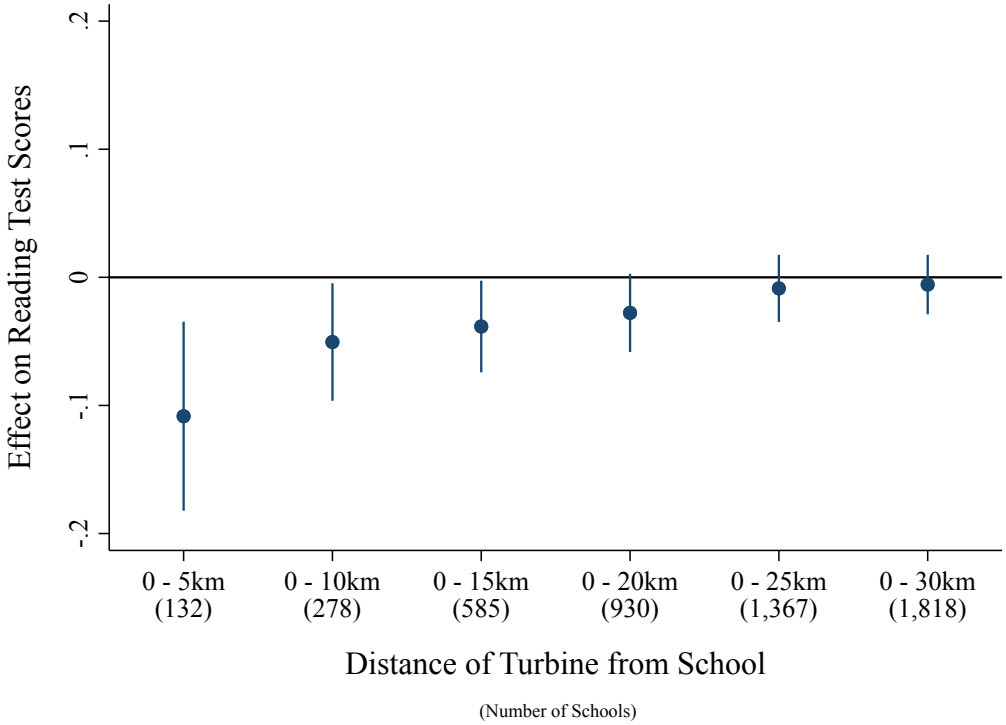
Note: The solid blue line presents the time series of the total installed wind energy capacity in the state of Texas. The dashed red line presents the annual number of new turbines installed. Data comes from the US Wind Turbine Database.

Figure 3.2: Wind Turbines and Public Schools



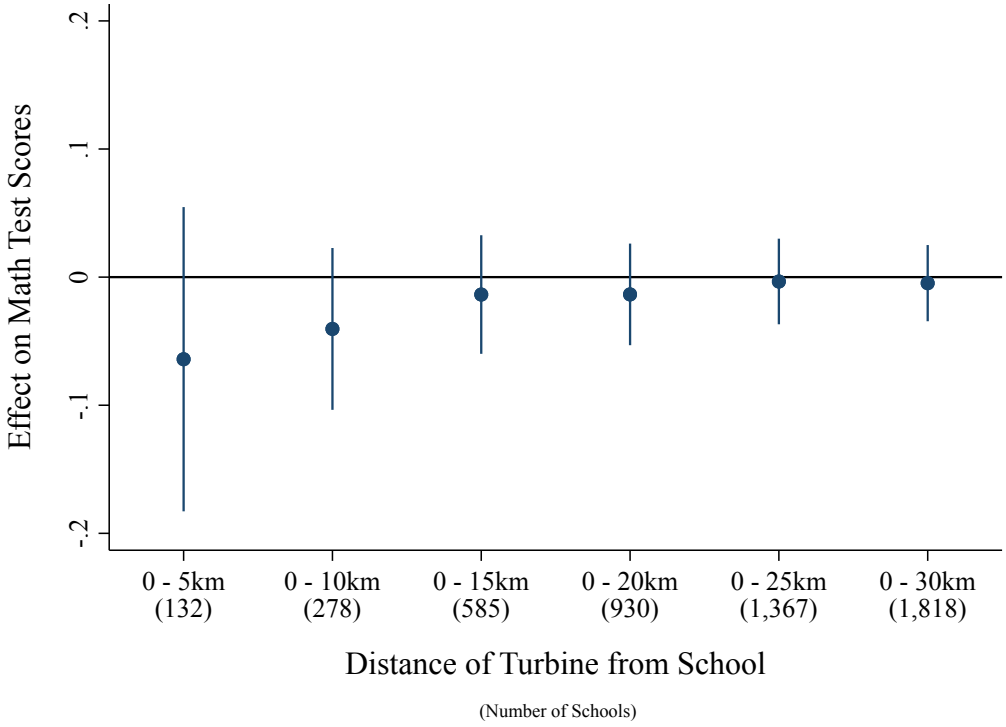
Note: Panel A shows the 15,424 wind turbines in the state of Texas. Many are located in the panhandle, western, and the southern parts of the state. Panel B show the wind turbines in selected counties in Central Texas and the public schools which are located in various distances near them. Overall, there are 51 schools within 30 km, including 13 within 10 km, of wind turbines in the 16 counties represented.

Figure 3.3: Impact of Wind Turbines on Reading Scores by distance



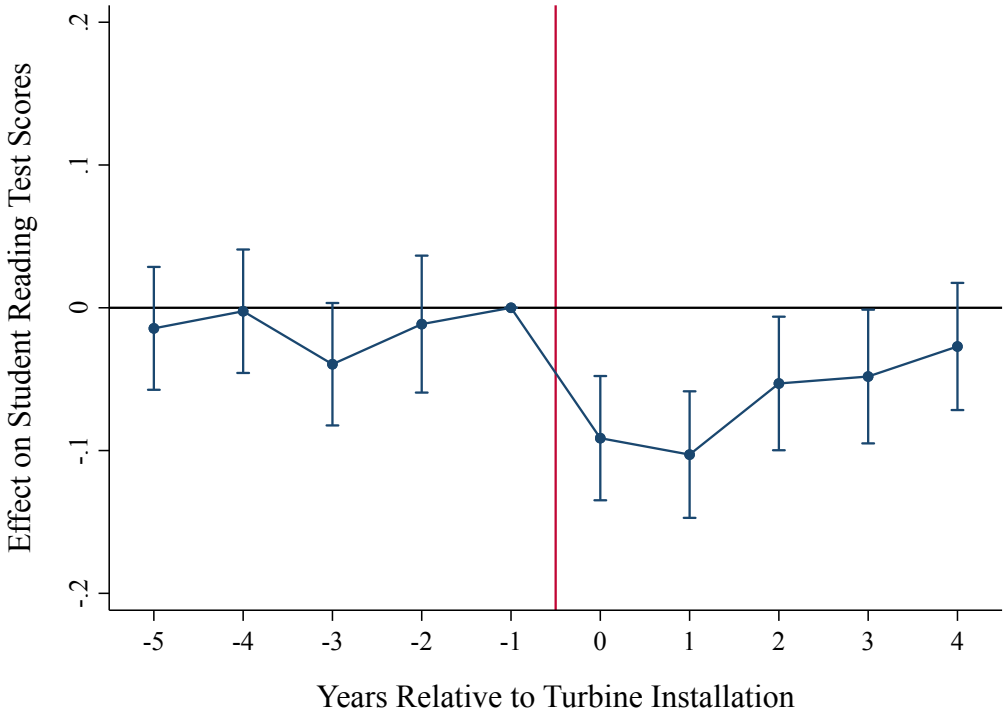
Note: The figure plots of the coefficients from the different regressions of the estimating equation 3.1 when the "Near" variable is changed from 5 km to 30 km. The vertical bars show 95% confidence intervals from standard errors clustered at the school level.

Figure 3.4: Impact of Wind Turbines on Math Scores by distance



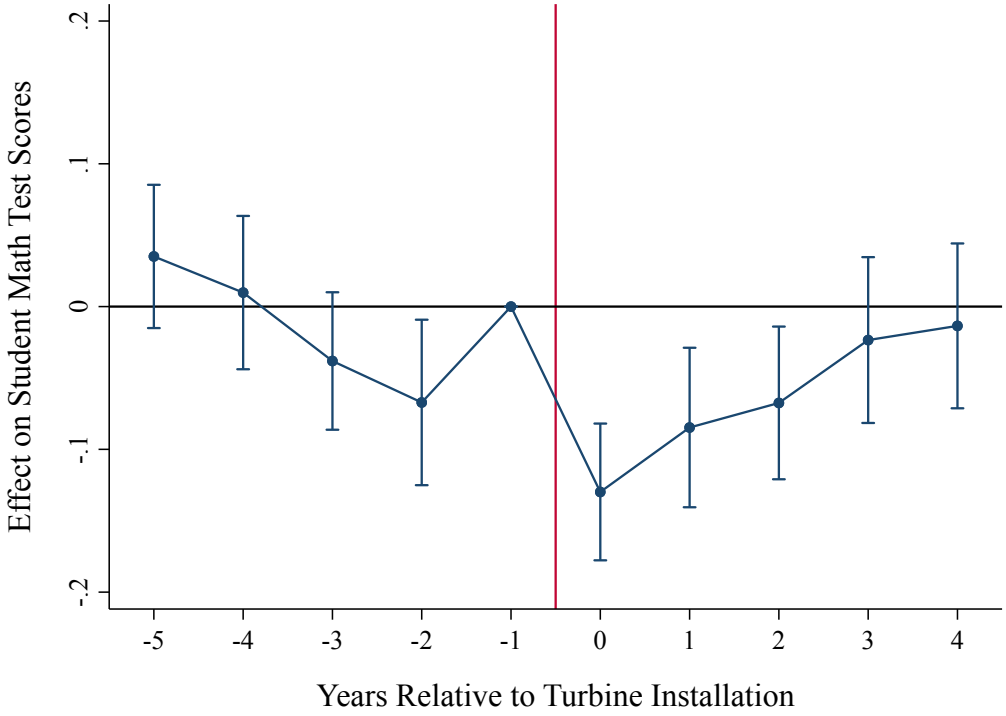
Note: The figure plots of the coefficients resulting from the different regressions of the estimating equation 3.1 when the "Near" variable is changed from 5 km to 30 km. The vertical bars show 95% confidence intervals from standard errors clustered at the school level.

Figure 3.5: Effect of Wind Turbines on Reading Scores within 10 km



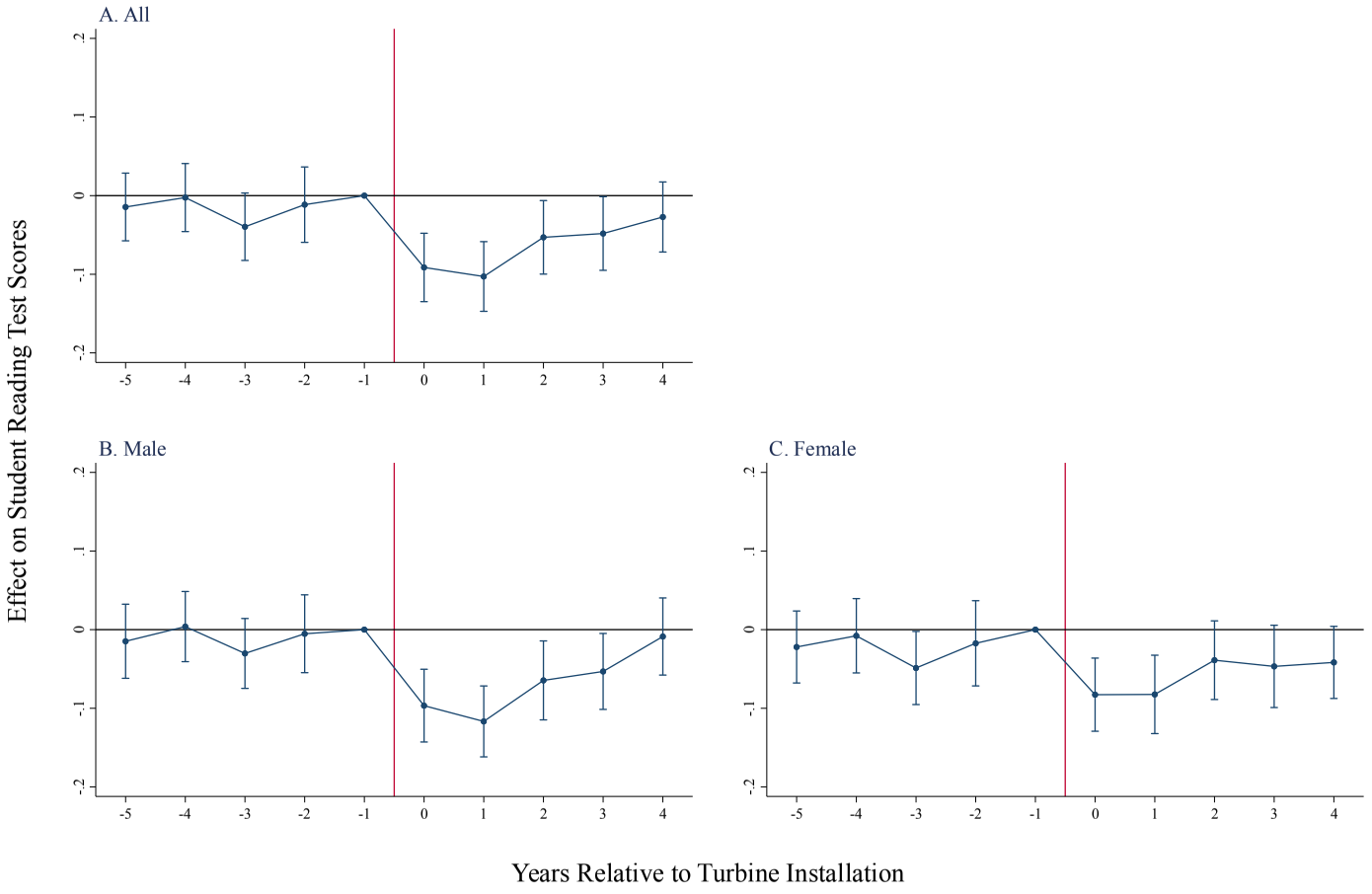
Note: The figure provides the plot of the yearly coefficients from equation 3.2 with the z-scores from reading standardized tests. The solid vertical line is immediately before relative year 0 when the first turbine was installed. The capped vertical bars show 95% confidence intervals from standard errors clustered at the school level. This plot provides evidence that the assumption of parallel pre-trends holds.

Figure 3.6: Effect of Wind Turbines on Math Scores within 10 km



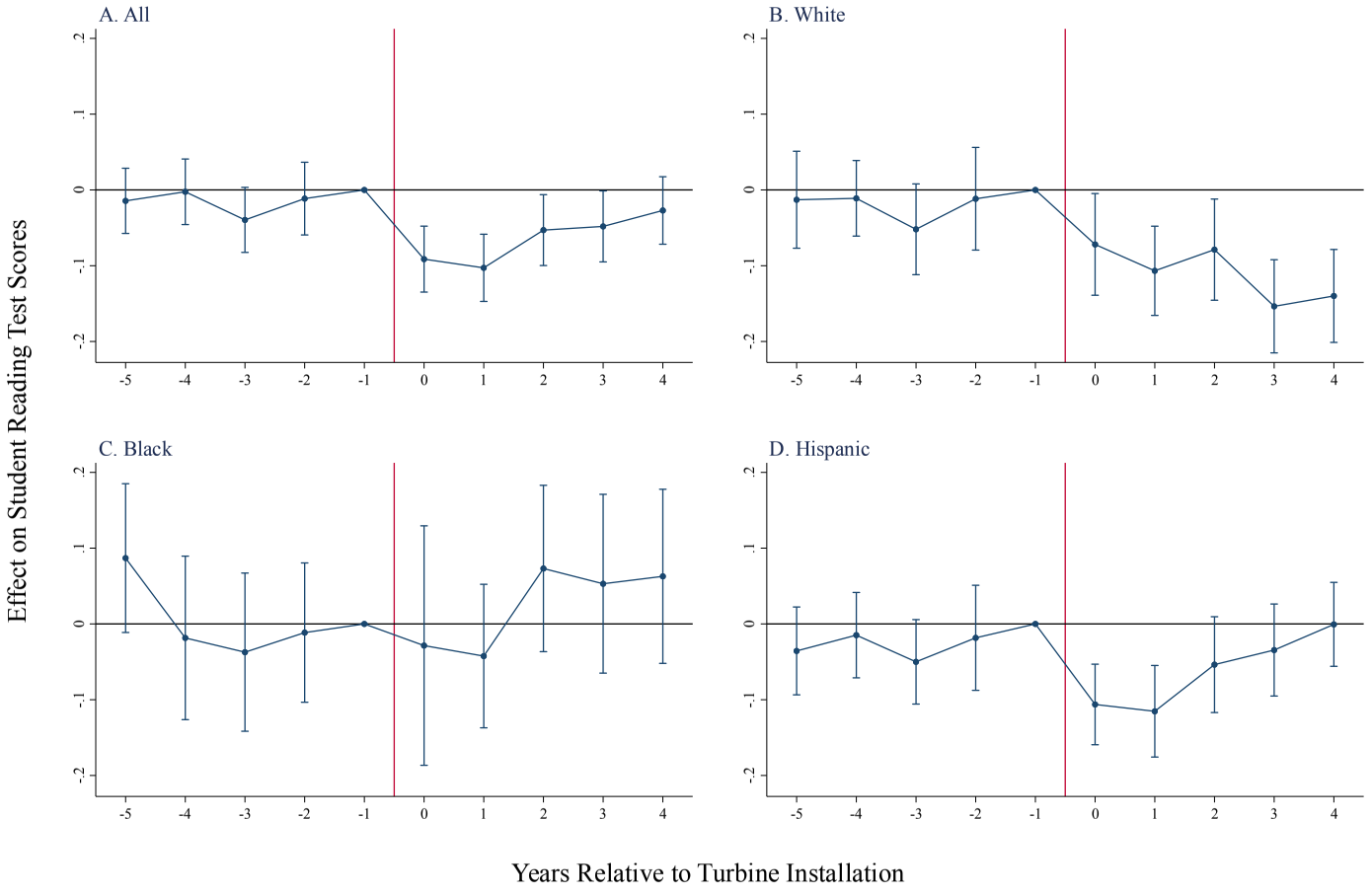
Note: The figure provides the plot of the yearly coefficients from equation 3.2 with the z-scores from math standardized tests. The solid vertical line is immediately before relative year 0 when the first turbine was installed. The capped vertical bars show 95% confidence intervals from standard errors clustered at the school level. Clearly from this plot, a linear trend is detected prior to the first turbine installation and the assumption of parallel pre-trends is violated.

Figure 3.7: Effect of Wind Turbines on Reading Scores by Gender within 10 km



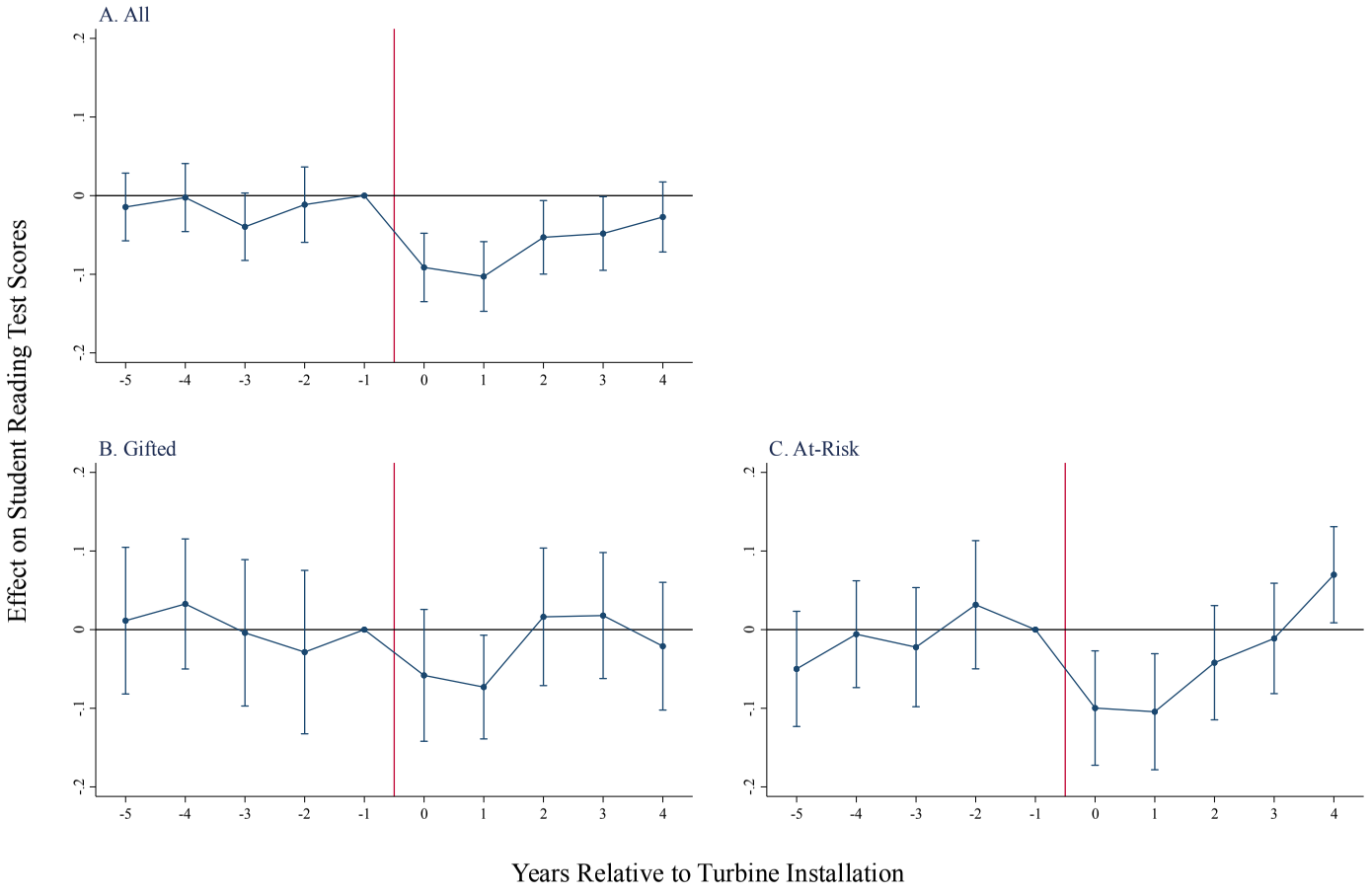
Note: Each panel provides the plot of the yearly coefficients from equation 3.2 with the z-scores from reading standardized tests by male and female students. The solid vertical line is immediately before relative year 0 when the first turbine was installed. The capped vertical bars show 95% confidence intervals from standard errors clustered at the school level.

Figure 3.8: Effect of Wind Turbines on Reading Scores by Ethnicity within 10 km



Note: Each panel provides the plot of the yearly coefficients from equation 3.2 with the z-scores from reading standardized tests for the labeled subgroups. The solid vertical line is immediately before relative year 0 when the first turbine was installed. The capped vertical bars show 95% confidence intervals from standard errors clustered at the school level.

Figure 3.9: Effect of Wind Turbines on Reading Scores by Academic Group within 10 km



Note: Each panel provides the plot of the yearly coefficients from equation 3.2 with the z-scores from reading standardized tests for gifted and at-risk students. The solid vertical line is immediately before relative year 0 when the first turbine was installed. The capped vertical bars show 95% confidence intervals from standard errors clustered at the school level.

Table 3.1: Summary Statistics: Texas Public Schools

	< 10 km: Before Installation (1)	< 10 km: After Installation (2)	All Others (3)	(1) - (3) Difference (4)
Number of Students	302	281	382	-80.46***
	(315)	(243)	(344)	(17.6)
Math z-score	-0.015	-0.073	-0.012	-0.002
	(0.767)	(1.02)	(0.868)	(0.026)
Reading z-score	0.045	-0.063	0.030	0.015
	(0.771)	(1.13)	(0.947)	(0.029)
Fraction male	0.501	0.508	0.506	-0.005***
	(0.033)	(0.046)	(0.035)	(0.001)
Fraction white	0.273	0.270	0.358	-0.085***
	(0.304)	(0.281)	(0.281)	(0.022)
Fraction Hispanic	0.685	0.662	0.453	0.232***
	(0.330)	(0.323)	(0.296)	(0.025)
Fraction econ. disadvantage	0.660	0.654	0.529	0.131***
	(0.285)	(0.273)	(0.274)	(0.023)
Fraction at risk	0.441	0.481	0.421	0.020
	(0.184)	(0.189)	(0.191)	(0.013)
Windmills within 10km	—	24.9	—	—
		(39.5)		
Number of Schools	338	354	6,809	

Note: All statistics are calculated at the school level and includes all school grades. Standard deviations are in parentheses. Demographic information are from students who took reading portion of the standardized tests from 2002-2019. Statistics are weighted by the number of students who took the tests. School data comes from the Texas Education Agency, and windmill data comes from the United States Wind Turbine Database.

Table 3.2: Impact of Wind Turbines on Student Achievement by Distance

	0-5 km (1)	5-10 km (2)	10-15 km (3)	15-20 km (4)	20-25 km (5)	25-30 km (6)
A. Reading Test Scores						
Post × Near	-0.108**	-0.020	-0.029	-0.016	0.031	0.004
	(0.038)	(0.028)	(0.026)	(0.024)	(0.021)	(0.022)
No. of obs.	266,302	268,318	268,512	268,727	270,716	269,682
B. Math Test Scores						
Post × Near	-0.064	-0.030	0.009	-0.033	0.017	0.000
	(0.061)	(0.037)	(0.031)	(0.032)	(0.027)	(0.029)
No. of obs.	272,585	274,684	274,888	275,112	277,191	276,056
N (Near schools)	132	246	307	345	437	451

Note: This includes all regular instructional schools in Texas from 2002-2018. Test scores are standardized to mean 0 and SD 1. School controls for all regressions include the fractions of students who are male, white, Hispanic, economically disadvantaged, and at-risk for dropping out. Grade, school, and year fixed effects included. "Post" indicates years after wind turbine installation. "Near" indicates schools that are within 10 km of turbines. Regressions are weighted by the number of students who took the tests. Standard errors are clustered at the school level. *** p<0.01, ** p <0.05, * p <0.1

Table 3.3: Effect of Wind Turbines on Student Achievement

	(1)	(2)
A. Reading Test Scores		
Post × Near	-0.046**	-0.043*
	(0.022)	(0.025)
No. of obs.	292,263	291,692
B. Math Test Scores		
Post × Near	-0.023	-0.039
	(0.030)	(0.030)
No. of obs.	299,617	298,956
C. Difference (A-B)		
	-0.023	-0.004
	(0.037)	(0.034)
Grade fixed effects	X	
School fixed effects	X	
Year fixed effects	X	X
Grade × School fixed effects		X

Note: This includes all regular instructional schools in Texas from 2002-2018. Test scores are standardized to mean 0 and SD 1. School controls for all regressions include the fractions of students who are male, white, Hispanic, economically disadvantaged, and at-risk for dropping out. "Post" indicates years after wind turbine installation. "Near" indicates schools that are within 10 km of turbines. Regressions are weighted by the number of students who took the tests. Standard errors are clustered at the school level. *** p<0.01, ** p <0.05, * p <0.1

Table 3.4: Effect of Wind Turbines on Test Scores by Gender

	All	Male	Female	M - F Difference
	(1)	(2)	(3)	(4)
A. Reading Test Scores				
Post × Near	-0.046**	-0.056**	-0.034	-0.022*
	(0.22)	(0.022)	(0.022)	(0.013)
p-value	[0.037]	[0.023]	[0.121]	[0.0871]
q-value	—	⟨0.049⟩	⟨0.065⟩	—
No. of obs.	293,263	286,144	285,824	—
B. Math Test Scores				
Post × Near	-0.023	-0.026	-0.019	-0.007
	(0.030)	(0.029)	(0.032)	(0.012)
p-value	[0.443]	[0.382]	[0.543]	[0.548]
q-value	—	⟨1.000⟩	⟨1.000⟩	—
No. of obs.	299,617	293,084	292,699	—

Note: This includes all regular instructional schools in Texas from 2002-2018. Test scores are standardized to mean 0 and SD 1. School controls for all regressions include the fractions of students who are male, white, Hispanic, economically disadvantaged, and at-risk for dropping out. Grade, school, and year fixed effects included. "Post" indicates years after wind turbine installation. "Near" indicates schools that are within 10 km of turbines. Regressions are weighted by the number of students who took the tests. Standard errors are clustered at the school level. P-values are included in square brackets. Benjamini, Krieger and Yekutieli (2006) sharpened q-values are included in angled brackets. *** p<0.01, ** p <0.05, * p <0.1

Table 3.5: Effect of Wind Turbines on Test Scores by Ethnicity

	All	White	Black	Hispanic	W - B Difference	W - H Difference
	(1)	(2)	(3)	(4)	(5)	(6)
A. Reading Test Scores						
Post × Near	-0.046**	-0.082**	-0.020	-0.026	-0.062*	-0.056
	(0.22)	(0.029)	(0.040)	(0.029)	(0.036)	(0.041)
p-value	[0.037]	[0.005]	[0.608]	[0.367]	[0.085]	[0.173]
q-value	—	⟨0.016⟩	⟨0.682⟩	⟨0.580⟩	—	—
No. of obs.	293,263	233,052	147,603	258,566	—	—
B. Math Test Scores						
Post × Near	-0.023	-0.089*	-0.044	0.031	-0.045	-0.120**
	(0.030)	(0.045)	(0.054)	(0.037)	(0.071)	(0.058)
p-value	[0.443]	[0.051]	[0.418]	[0.399]	[0.528]	[0.039]
q-value	—	⟨0.180⟩	⟨0.387⟩	⟨0.387⟩	—	—
No. of obs.	299,617	233,052	149,417	264,501	—	—

Note: This includes all regular instructional schools in Texas from 2002-2018. Test scores are standardized to mean 0 and SD 1. School controls for all regressions include the fractions of students who are male, white, Hispanic, economically disadvantaged, and at-risk for dropping out. Grade, school, and year fixed effects included. "Post" indicates years after wind turbine installation. "Near" indicates schools that are within 10 km of turbines. Regressions are weighted by the number of students who took the tests. Standard errors are clustered at the school level. P-values are included in square brackets. Benjamini, Krieger and Yekutieli (2006) sharpened q-values are included in angled brackets. *** p<0.01, ** p <0.05, * p <0.1

Table 3.6: Effect of Wind Turbines on Test Scores by Academic Group

	All	Gifted	At-Risk	Gifted - At-Risk Difference
	(1)	(2)	(3)	(4)
A. Reading Test Scores				
Post × Near	-0.046**	-0.074**	0.028	-0.102**
	(0.22)	(0.037)	(0.032)	(0.049)
p-value	[0.037]	[0.046]	[0.379]	[0.037]
q-value	—	⟨0.101⟩	⟨0.214⟩	—
No. of obs.	293,263	186,661	271,902	—
B. Math Test Scores				
Post × Near	-0.023	0.012	0.054	-0.042
	(0.030)	(0.046)	(0.036)	(0.059)
p-value	[0.443]	[0.800]	[0.138]	[0.471]
q-value	—	⟨0.667⟩	⟨0.382⟩	—
No. of obs.	299,617	293,084	275,933	—

Note: This includes all regular instructional schools in Texas from 2002-2018. Test scores are standardized to mean 0 and SD 1. School controls for all regressions include the fractions of students who are male, white, Hispanic, economically disadvantaged, and at-risk for dropping out. Grade, school, and year fixed effects included. "Post" indicates years after wind turbine installation. "Near" indicates schools that are within 10 km of turbines. Regressions are weighted by the number of students who took the tests. Standard errors are clustered at the school level. P-values are included in square brackets. Benjamini, Krieger and Yekutieli (2006) sharpened q-values are included in angled brackets. *** p<0.01, ** p <0.05, * p <0.1

Appendices

Appendix A

Robustness Checks

A.1 Alternative Thresholds for Treatment

Table A.1 provides the detailed list of the paper's preferred specification of treated dioceses with the corresponding number of first-time allegations in 2002. One worry is whether the results rely on the specific threshold for a treated diocese in order to achieve the results of the paper. Figure A.1 shows the event study plots for log infant baptism for several different thresholds: greater than 3, 5, 7, and 9 allegations. Each of the plots follow a similar pattern to the preferred threshold of this paper, i.e. a high allegation diocese had more than 5 allegations in 2002. Additionally, Figure A.2 displays the coefficient plots for the log Catholic population, and a similar conclusion can be made.

Table A.2 presents the estimates of the effect on the Sacraments of Initiation with high allegation threshold of more than 3 allegations in the even columns. Under this new threshold, 73 of the 176 dioceses are treated. The estimates from the original specification are in the odd columns. The estimates from the altered threshold are slightly smaller but qualitatively similar to the those of the preferred specification. For instance, under this new specification,

First Communions drop by 6.9 percent as opposed to 7.8 percent. Additionally, Table A.3 similarly has the results for the effect on mortality related to despair for those aged 55 to 64. Again, there are qualitatively similar results for each cause of death although the estimates somewhat smaller. The incidence of a high suicide mortality rate rises by 1.2 percentage points under this specification as opposed to the 1.8 percentage points estimated using the original threshold. Overall, the analysis of this paper is robust when the threshold is changed.

Finally, Table A.4 presents the estimates of the effect on the Sacraments of Initiation when the measure of the intensity of the scandal is measured in terms of the per capita number of first-time allegations. Under this measure, I consider the top 50 dioceses as being treated. The estimates on each participation variable increases when compared to the baseline specification. For instance, infant baptisms drop by 16.3 percent compared to 13.2 percent.

A.2 Alternative Outcome Specification

One concern with this identification of this paper is the specification of the outcome variables. For the preferred specification, I have used the log of outcomes. One suggestion is to use per capita measures because that will take in to account the different sizes of the dioceses which varies substantially.

As such, the paper does this for outcomes per one million people. I find qualitatively similar results to the preferred specification. Figure A.3 presents the corresponding plots of the year coefficients for infant baptisms and First

Communion. Table A.5 presents the pooled estimates for the effect on these sacraments. We recall that the mean total population for high allegation dioceses before the scandal was 2.37 million. Then, from columns 2 and 4, the average high allegation diocese had declines of about 776 infant baptisms and 673 First Communions, which are very close to the previous estimates.

The pattern holds the for diocesan schools. Figures A.4 and A.5 display the respective coefficient plots for elementary and high schools. In Table A.6, the estimates in Panel A show that the average high allegation diocese experienced a loss of nearly 10.9 elementary schools, Similarly, the average affected diocese loses more than 3,573 students, which is 16.2 percent of the 2001 average enrollment. From Panel B, we find that the decline is about 0.64 high schools and 366 students.

A.3 Different Data Set for Catholic Schools

As an additional check on the results related to enrollment at Catholic schools, I obtain data from the National Catholic Educational Association on the number of schools and students that attend both diocesan and Catholic private schools. Table A.7 shows the estimates on all Catholic schools using this data set. The estimates are very similar to those from the OCD presented in Table 1.6. As a reminder, we do not see a significant effect on high school enrollment since this data does not disaggregate between diocesan and private schools.

A.4 Parametric Event Study

With respect to some participation variables, some might be concerned that there are existing trends that violate the standard parallel trends assumption of the difference-in-differences model. In order to remedy this, I follow Dobkin et al. (2018) and estimate a more parametric model that directly controls for pre-existing trends:

$$Y_{dt} = \zeta_0 + \sum_{T>2001} [\zeta_{1T} Treat_d \times \mathbb{1}(t = T)] + \sum_{D} [\upsilon_D \times \mathbb{1}(d = D) \cdot t] + \zeta_2 X_{dt} + \lambda_d + \psi_t + \omega_{dt} \tag{A.1}$$

The main differences between this and Equation 1.2 are twofold. Diocesan linear time trends are included as $\sum_D [\upsilon_D \times \mathbb{1}(d = D) \cdot t]$ and the coefficients on the interaction of $Treat_d$ with time fixed effects are only estimated for the post period. Under this model, the identifying assumption holds that absent the 2002 scandal, any pre-existing differential trends in the outcome variables across dioceses would have continued as before.

Since the only estimates the interaction terms for the post-period, the ζ_{1T} do not give any good indication of whether the linear pre-trend fits the data. As such, I follow Beheshti (2019) to rescale the β coefficients from Equation 1.2 to equal the ζ coefficients when presenting the results. One can then examine if there are any deviations from the linear pre-trend by seeing if the new β coefficients differ from zero in the pre-period.

Figure A.6 provides both the original and the parametric event study plots for the religious participation variables of infant baptism and First Com-

munion. Comparing the two event studies for infant baptism in Panels A and B, the reader finds that the coefficients deviate less from zero in the pre-period when controlling for a linear pre-trend. Additionally, while the effect may be attenuated, there are still statistically significant effects in the post-period. For instance, the estimate for the rescaled $\beta_{1,2006}$ implies a 7% decline in infant baptisms. Similarly, one can say the same about the parametric event study plot for log Catholic population in Panel B of A.7 when comparing it to the original plot in Panel A. Additionally, Figure A.8 presents the event study plots for the effect on the log number of diocesan schools and student enrollment at the high school level. In Panel D, the event study plot suggests that the effect on high school enrollment is attenuated and lags more than in the original specification. Overall, the paper finds that these plots closely resemble those resulting from the preferred specification of Equation 1.2.

A.5 Matching on Covariates

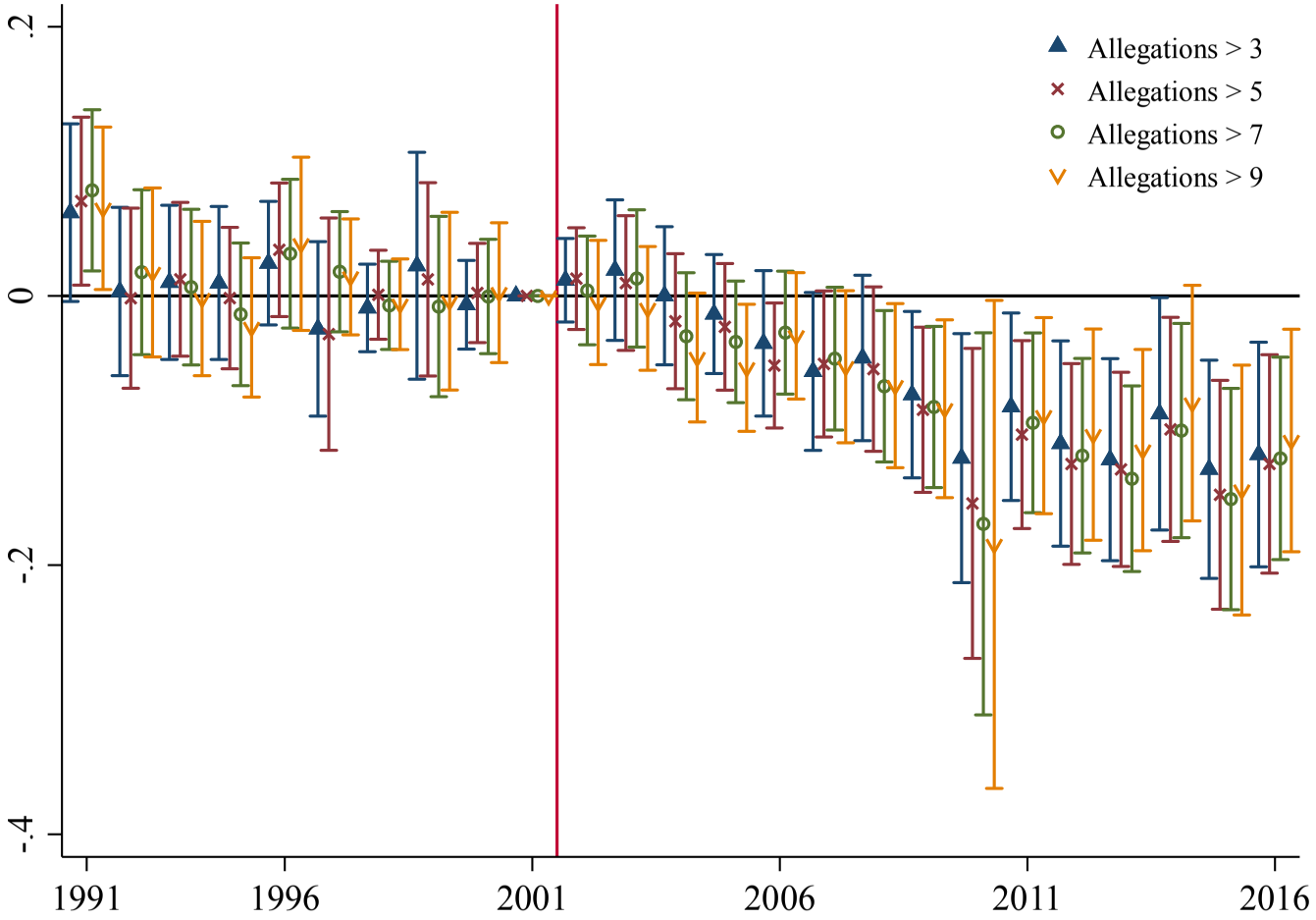
Another concern is that there is a a gulf when comparing the means of dependent variables between treated and control dioceses in 2001 (Table 1.1). In an effort to remedy this, I use kernel propensity-score matching on the common support.[1] I match on the following covariates: the number of first-time allegations, Hispanic percentage of the diocese, per capita income, and population size. Table A.8 provides a summary of the same statistics when propensity score matching is employed. Some of the largest treated

[1] I use the Epanechnikov kernel function and a kernel bandwidth of 0.06

dioceses are dropped, including Boston, Chicago, Los Angeles, and New York. Additionally, we find that the means of the dependent variables are more balanced between high and low allegation dioceses than previously. Tables A.9, A.10, and A.11 provide the estimates of the effect on the different participation outcomes. The estimates are largely similar to the original estimates. For instance, infant baptisms decline by 10.6 percent which is a slightly larger impact of the 7.9 percent estimated using the preferred specification.

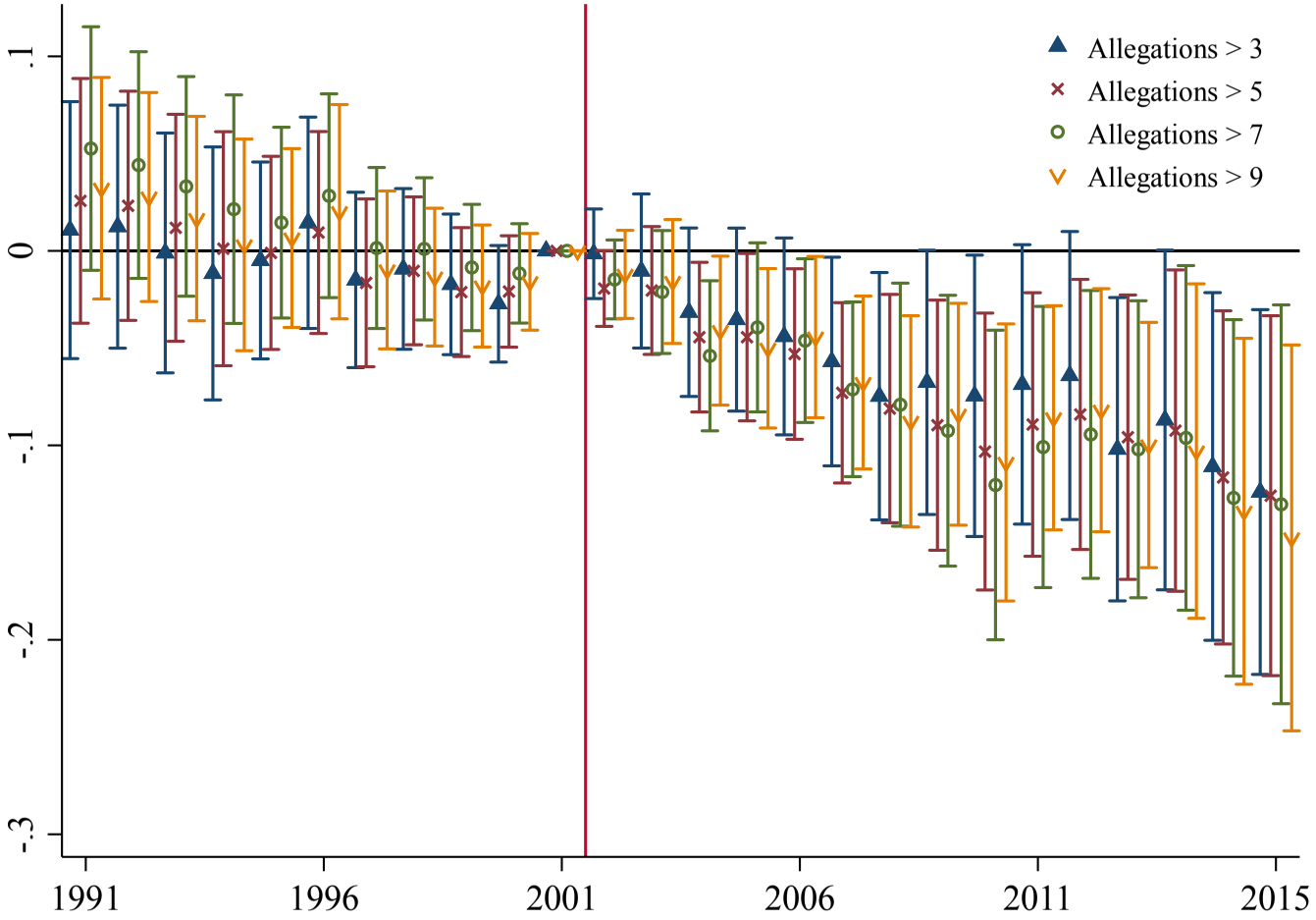
Additionally, there are similar findings when matching on county characteristics for the despair analysis. Table A.12 shows a more comparable analysis when matching on the Hispanic percentage, Catholic percentage, per capita income, and unemployment rate. In Table A.13, the estimates are reduced but still statistically significant among men for causes related to accidental overdoses and liver disease.

Figure A.1: Effect on Log Infant Baptisms (Various Thresholds)



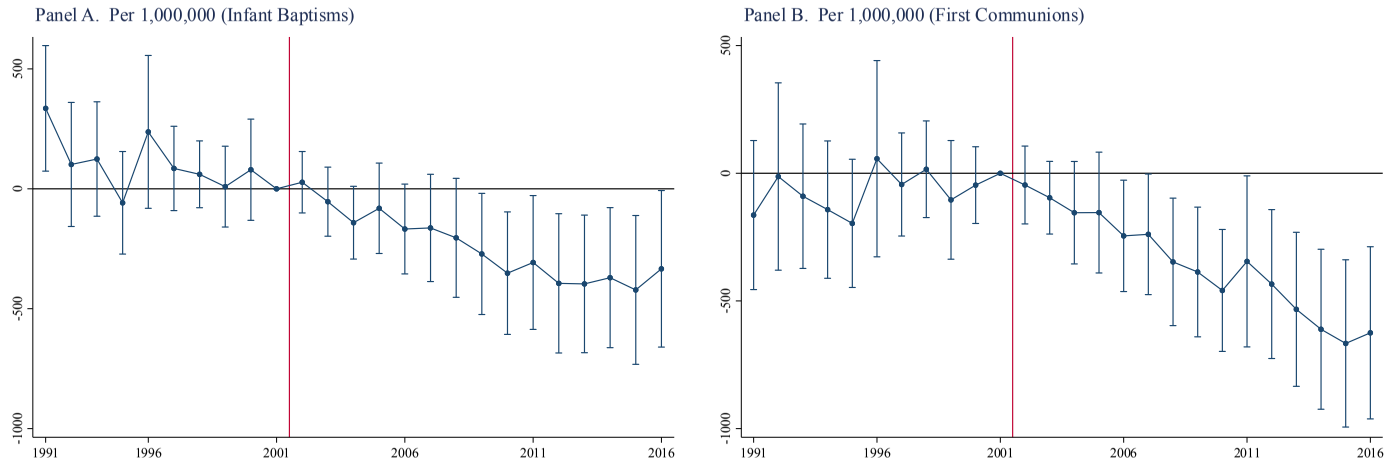
Note: Note: This is the plot of the estimated yearly coefficients from Equation 1.2 for the log number of infant baptisms using different treatment thresholds. The solid vertical line is immediately before 2002 when the abuse scandal first broke nationally. The capped vertical bars show 95% confidence intervals from standard errors clustered at the diocesan level. These plots provide evidence that the assumption of parallel pre-trends hold and that there is a difference in how the two types of dioceses evolved after the scandal.

Figure A.2: Effect on Log Catholic Population (Various Thresholds)



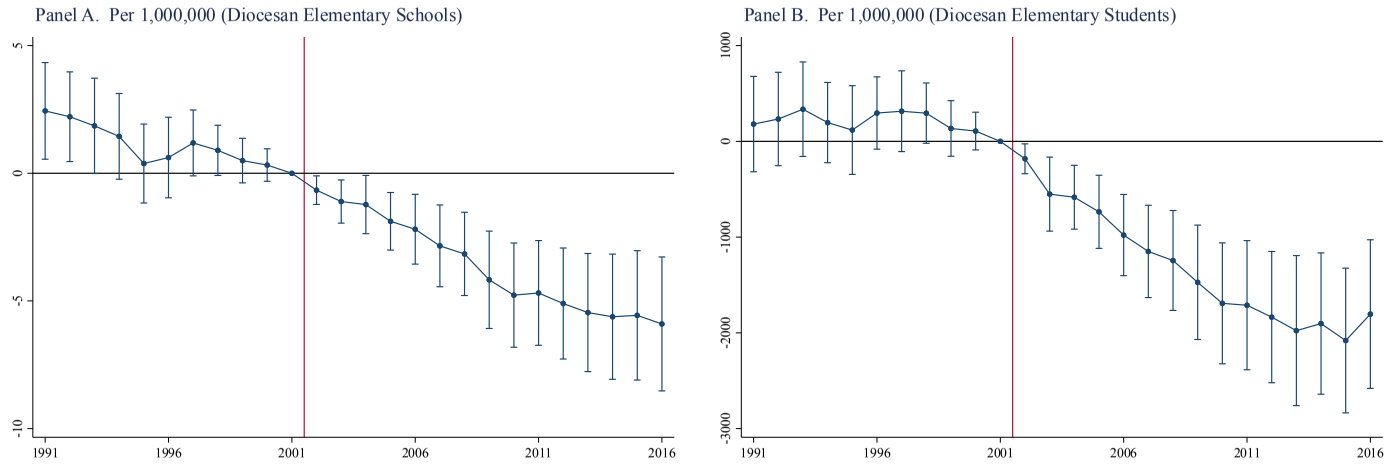
Note: Note: This is the plot of the estimated yearly coefficients from Equation 1.2 for the log number of infant baptisms using different treatment thresholds. The solid vertical line is immediately before 2002 when the abuse scandal first broke nationally. The capped vertical bars show 95% confidence intervals from standard errors clustered at the diocesan level. These plots provide evidence that the assumption of parallel pre-trends hold and that there is a difference in how the two types of dioceses evolved after the scandal.

Figure A.3: Effect on Infant Baptisms and First Communions in the US (Per 1 Million)



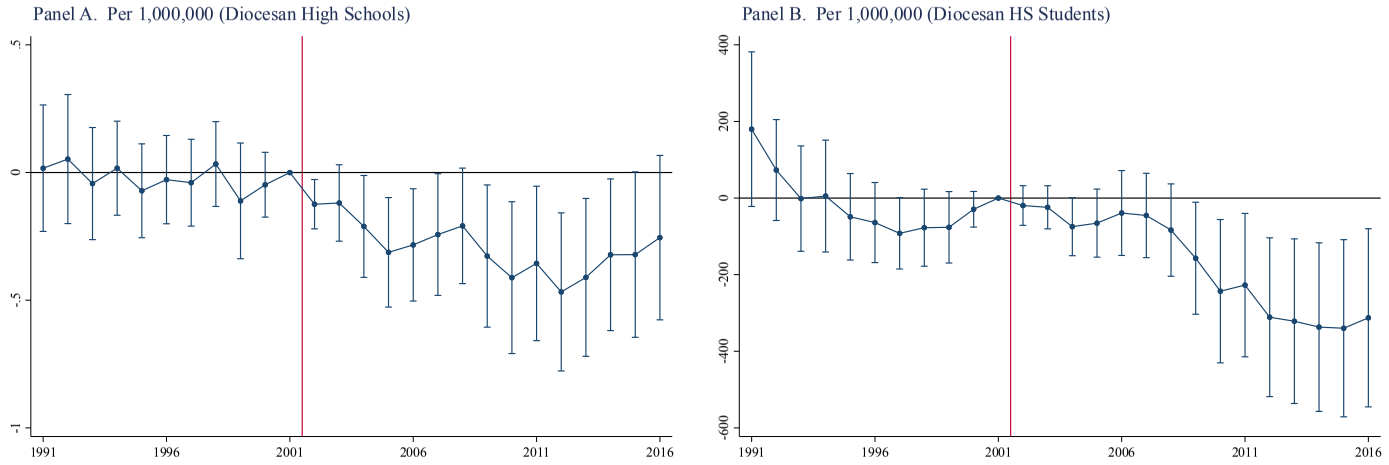
Note: The left panel provides the plots of the yearly coefficients from equation 1.2 for the number of infant baptisms per 1,000,000. The right panel presents the coefficient plot for the corresponding measure of the number of First Communions. In each panel, the solid vertical line is immediately before 2002 when the abuse scandal first broke nationally. The capped vertical bars show 95% confidence intervals from standard errors clustered at the diocesan level. These plots provide evidence that the assumption of parallel pre-trends hold and that there is a difference in how the two types of dioceses evolved after the scandal.

Figure A.4: Effect on Diocesan Elementary Schools and Students in the US (Per 1 Million)



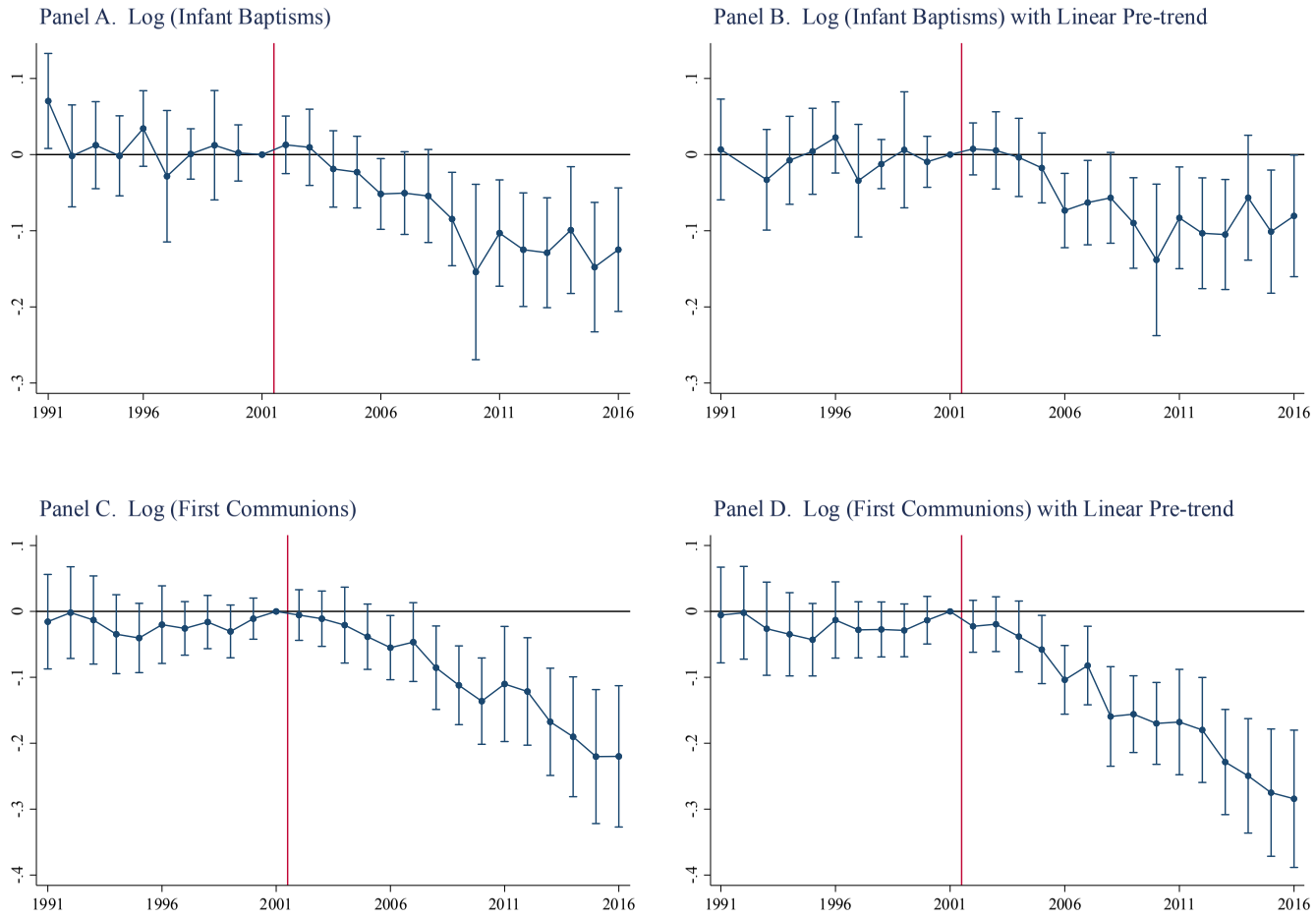
Note: The left panel provides the plots of the yearly coefficients from equation 1.2 for the number of elementary schools under diocesan control per 1,000,000. The right panel presents the coefficient plot for the corresponding measure of enrollment at diocesan elementary schools. In each panel, the solid vertical line is immediately before 2002 when the abuse scandal first broke nationally. The capped vertical bars show 95% confidence intervals from standard errors clustered at the diocesan level. These plots provide evidence that the assumption of parallel pre-trends hold and that there is a difference in how the two types of dioceses evolved after the scandal.

Figure A.5: Effect on Diocesan High Schools and Student Enrollment in the US (Per 1 Million)



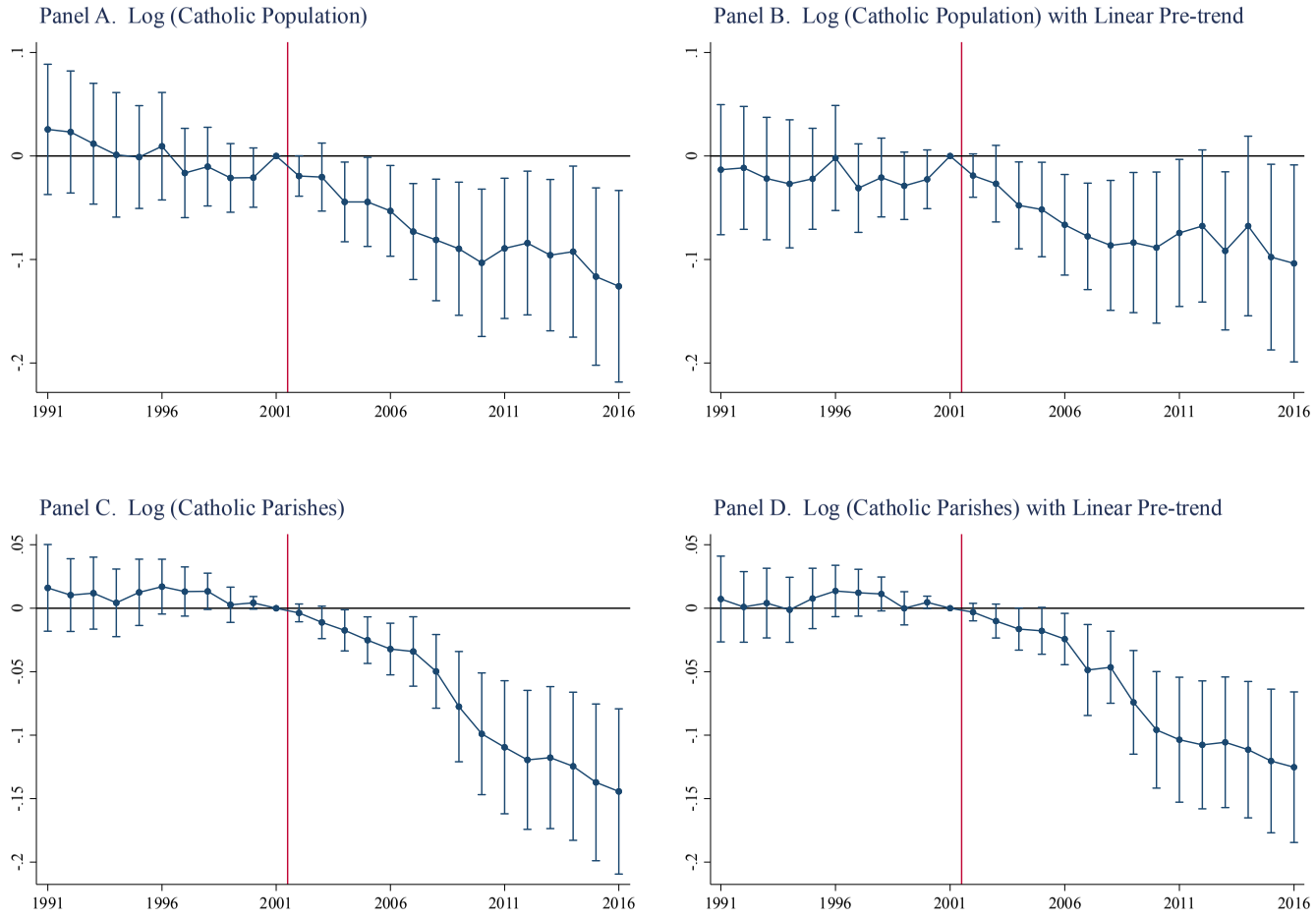
Note: The left panel provides the plots of the yearly coefficients from equation 1.2 for the number of high schools under diocesan control per 1,000,000. The right panel presents the coefficient plot for the corresponding mesasure of total enrollment at diocesan high schools. In each panel, the solid vertical line is immediately before 2002 when the abuse scandal first broke nationally. The capped vertical bars show 95% confidence intervals from standard errors clustered at the diocesan level. These plots provide evidence that the assumption of parallel pre-trends hold and that there is a difference in how the two types of dioceses evolved after the scandal.

Figure A.6: Effect on Infant Baptisms and First Communions



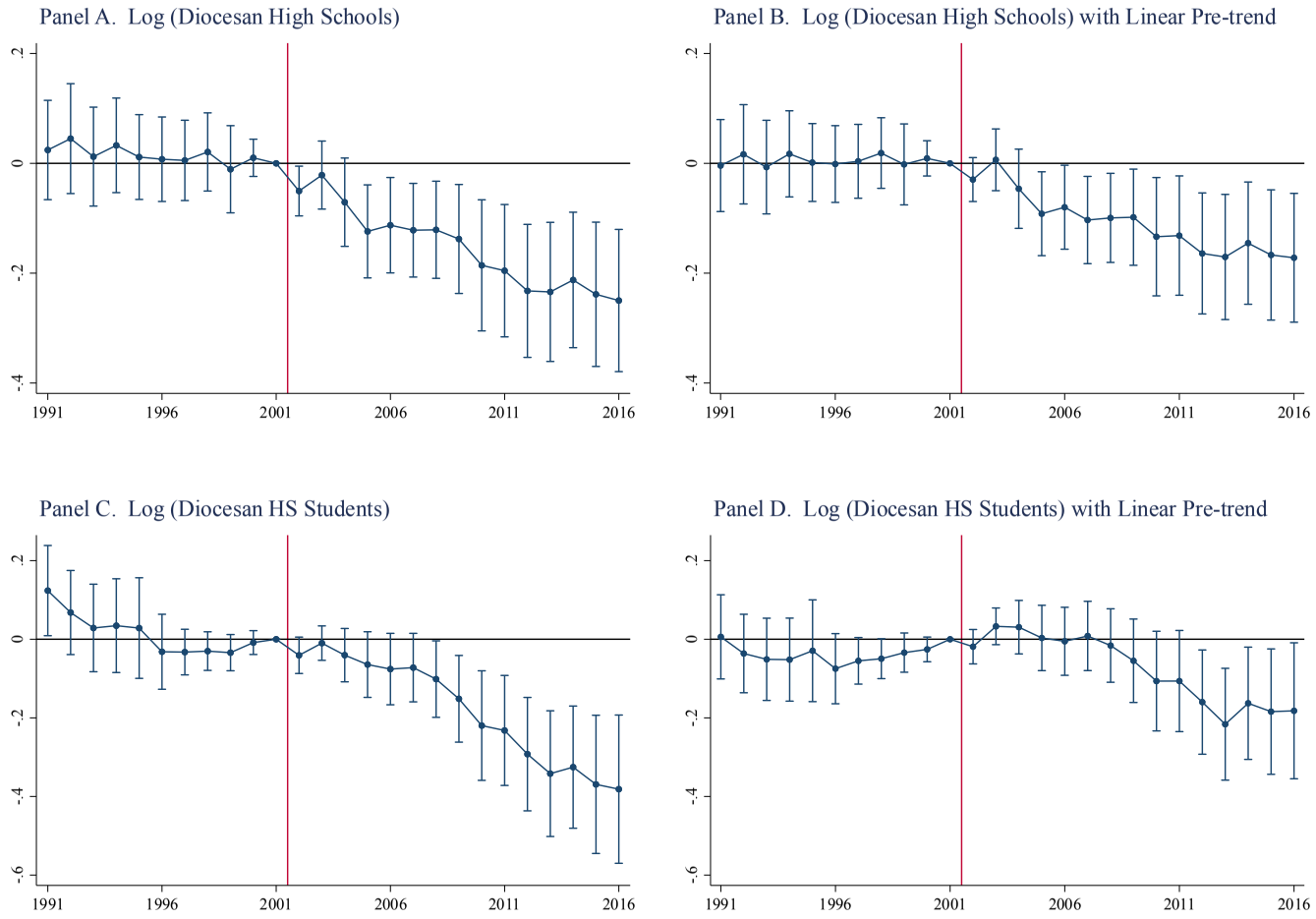
Note: The left column's event study plots are the same from Figure 1.4. The panels in the right column provide the plots of the yearly coefficients from equation 1.2 when controlling for a linear pre-trend. In each panel, the solid vertical line is immediately before 2002 when the abuse scandal first broke nationally. The capped vertical bars show 95% confidence intervals from standard errors clustered at the diocesan level. We see that in the post-period there are still statistically significant effects.

Figure A.7: Effect on Catholic Population and Parish Churches



Note: The left column's event study plots are the same from Figure 1.6. The panels in the right column provide the plots of the yearly coefficients from equation 1.2 when controlling for a linear pre-trend. In each panel, the solid vertical line is immediately before 2002 when the abuse scandal first broke nationally. The capped vertical bars show 95% confidence intervals from standard errors clustered at the diocesan level. We see that in the post-period there are still statistically significant effects.

Figure A.8: Effect on Diocesan High Schools and Student Enrollment



Note: The left column's event study plots are the same from Figure 1.8. The panels in the right column provide the plots of the yearly coefficients from equation 1.2 when controlling for a linear pre-trend. In each panel, the solid vertical line is immediately before 2002 when the abuse scandal first broke nationally. The capped vertical bars show 95% confidence intervals from standard errors clustered at the diocesan level. We see that in the post-period there are still statistically significant effects.

Table A.1: Allegations by High Allegation Dioceses in 2002

Diocese	State	Allega-tions	Diocese	State	Allega-tions
Boston	MA	103	Altoona-Johnstown	PA	10
Manchester	NH	45	Paterson	NJ	10
Baltimore	MD	34	San Diego	CA	10
Los Angeles	CA	34	Tucson	AZ	10
Louisville	KY	32	Grand Rapids	MI	9
Cleveland	OH	29	GreenBay	WI	9
Detroit	MI	25	Newark	NJ	9
Rockville Centre	NY	22	Springfield	MA	9
New York	NY	21	St. Petersburg	FL	9
Chicago	IL	20	Peoria	IL	8
Portland	OR	20	Sacramento	CA	8
Fall River	MA	19	Syracuse	NY	8
St. Cloud	MN	19	Camden	NJ	7
Toledo	OH	17	Hartford	CT	7
Brooklyn	NY	15	Portland	ME	7
Joliet	IL	14	Seattle	WA	7
Milwaukee	WI	14	St. Paul and Minn.	MN	7
Philadelphia	PA	14	Allentown	PA	6
St. Louis	MO	14	Burlington	VT	6
Bridgeport	CT	13	Cincinnati	OH	6
Rochester	NY	12	Evansville	IN	6
Worcester	MA	12	Lansing	MI	6
Miami	FL	11	Richmond	VA	6
San Francisco	CA	11	San Jose	CA	6
Albany	NY	10	Spokane	WA	6

Note: This table displays the number of Catholic officials who for first-time were accused of sexual abuse in the 50 high allegation dioceses in 2002. Data comes from Bishop Accountability.

Table A.2: Effect of High Allegations on Church Vitals (Threshold > 3 Allegations)

	Infant Baptism		**First Communion**		**Confirmation**	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	-0.0821**	-0.0703**	-0.0812**	-0.0716**	-0.0023	-0.0489
	(0.0318)	(0.0332)	(0.0338)	(0.0339)	(0.0465)	(0.0478)
>5 allegation	X		X		X	
>3 allegation		X		X		X
No. of obs.	4,380	4,380	4,547	4,547	4,556	4,556

Note: The outcome variable is log of the labeled participation outcome. Standard errors are clustered at the diocesan level. Diocesan controls for all regressions include percentage of diocesan population that is Hispanic, per capita income, and, for Baptism, the percentage of the population that is infant aged. *** p <0.01, ** p <0.05, * p <0.1

Table A.3: Effect of High Allegations on Incidence of Counties with Mortality Rate ≥10 per 100,000 (Threshold > 3 Allegations), Aged 55 to 64

	Male		Female	
	(1)	(2)	(3)	(4)
A. Accidental Overdose				
Treat × Post	0.0117*	0.0085*	0.0072	0.0043
	(0.0069)	(0.0047)	(0.0045)	(0.0029)
> 5 allegations	X		X	
> 3 allegations		X		X
No. of obs.	65,524	65,524	65,524	65,524
B. Suicide				
Treat × Post	0.0175**	0.0118**	0.0008	0.0007
	(0.0071)	(0.0051)	(0.0018)	(0.0012)
> 5 allegations	X		X	
> 3 allegations		X		X
No. of obs.	65,524	65,524	65,524	65,524
C. Alcoholic Liver Disease				
Treat × Post	0.0200**	0.0181**	-0.0005	0.0004
	(0.0082)	(0.0062)	(0.0035)	(0.0025)
> 5 allegations	X		X	
> 3 allegations		X		X
No. of obs.	65,524	65,524	65,524	65,524

Note: The outcome variables are indicators whether a county experienced a mortality rate greater than 10 per 100,000 for each of the individual death categories. Data comes from the CDC Wonder database for the years 1991-2015. County and year fixed effects are included. Standard errors are clustered at the diocesan level. County controls include percentage of population that is Hispanic, per capita income, manufacturing employment, and unemployment rate. *** p <0.01, ** p <0.05, * p <0.1

Table A.4: Effect of High Allegations on Church Vitals (Allegations Per Capita)

	Infant Baptism		**First Communion**		**Confirmation**	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	-0.0821**	-0.1467***	-0.0812**	-0.1431**	-0.0023	-0.1175**
	(0.0318)	(0.0294)	(0.0338)	(0.0339)	(0.0400)	(0.0545)
Number	X		X		X	
Per Capita		X		X		X
No. of obs.	4,380	4,380	4,547	4,547	4,556	4,556

Note: The outcome variable is log of the labeled participation outcome. The odd columns present the estimates for the baseline specification that uses the number of first-time allegations as a measure of the exposure to the scandal. The even columns present the estimates when the level of the intensity is measured by the per capita number of first-time allegations. Standard errors are clustered at the diocesan level. Diocesan controls for all regressions include percentage of diocesan population that is Hispanic, per capita income, and, for Baptism, the percentage of the population that is infant aged. *** p <0.01, ** p <0.05, * p <0.1

Table A.5: Effect of High Allegations on Church Vitals (Per 1,000,000)

	Infant Baptism		First Communion		Confirmation	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	-631.4***	-327.5**	-351.6***	-284.1**	-12.7	39.6
	(142.8)	(135.9)	(112.8)	(101.8)	(114.8)	(107.4)
Controls		X		X		X
No. of obs.	4,379	4,379	4,545	4,545	4,554	4,554

Note: The outcome variable is the labeled participation outcome per capita scaled up by 1,000,000. The total annual population of those residing in the diocesan boundaries is taken from the OCD. Diocesan and year fixed effects are included. Standard errors are clustered at the diocesan level. Diocesan controls for all regressions include percentage of diocesan population that is Hispanic, per capita income, and, for Baptism, the percentage of the population that is infant aged. *** p <0.01, ** p <0.05, * p <0.1

Table A.6: Effect of High Allegations on Diocesan Schools (Per 1,000,000)

	Number of Schools		Student Enrollment	
	(1)	(2)	(3)	(4)
A. Elementary Schools				
Treat × Post	-4.78***	-4.62***	-1611***	-1508***
	(1.14)	(1.16)	(330.1)	(328.9)
Controls		X		X
No. of obs.	4,537	4,537	4,539	4,539
B. High Schools				
Treat × Post	-0.3260***	-0.2682**	-184.5***	-154.6**
	(0.1259)	(0.1173)	(68.7)	(69.1)
Controls		X		X
No. of obs.	4,106	4,106	4,107	4,107

Note: The outcome variable is is the labeled outcome per capita scaled up by 1,000,000. The total annual population of those residing in the diocesan boundaries is taken from the OCD. Diocesan and year fixed effects are included. Standard errors are clustered at the diocesan level. Diocesan controls include percentage of diocesan population that is Hispanic and per capita income. *** p <0.01, ** p <0.05, * p <0.1

Table A.7: Effect of High Allegations on Catholic Schools (NCEA)

	Number of Schools		Student Enrollment	
	(1)	(2)	(3)	(4)
A. Elementary Schools				
Treat × Post	-0.1197***	-0.1208***	-0.1370***	-0.1338***
	(0.0298)	(0.0292)	(0.0299)	(0.0304)
Controls		X		X
No. of obs.	3,312		3,313	
B. High Schools				
Treat × Post	-0.0674**	-0.0588**	-0.0505	-0.0441
	(0.0285)	(0.0277)	(0.0337)	(0.0318)
Controls		X		X
No. of obs.	3,144		3,144	

Note: The outcome variable is the log number of the labeled participation outcome for all Catholic schools, i.e. both diocesan and private. Diocesan and year fixed effects are included. Standard errors are clustered at the diocesan level. Diocesan controls include percentage of diocesan population that is Hispanic and median household income. Data comes from the NCEA for years 1997-2016. Using this secondary source of data, we find that the effects on enrollment are attenuated when we combine diocesan and private school data together. In order to get a clearer effect on the different types of schools, NCES data is used, and results are presented in Table 1.5. *** p <0.01, ** p <0.05, * p <0.1

Table A.8: Averages of Key Church Variables by Matched High and Low Allegation Dioceses in 2001

	High Allegation	**Low Allegation**
No. of Dioceses	43	76.1
No. of Allegations	0.26	0.23
No. of Baptisms	7,347	5,418
No. of First Communions	7,176	4,879
No. of Confirmations	5,317	3,397
No. of Elementary Schools	61.2	31.55
No. of Elementary Students	18,290	9,365
No. of High Schools	6.09	3.57
No. of HS Students	3,522	1,930
No. of Parish Churches	148.3	92.8
Catholic Population	534,857	319,398
Total Population	1,977,537	2,173,741

Note: The table shows the means of key participation variables in 2001, one year before the abuse scandal when using propensity score matching on the common support. Matched covariates include total population, percent Hispanic, per capita income, and number of allegations. Allegation data comes from Bishop Accountability. All other data is from the OCD.

Table A.9: Effect of High Allegations on Church Vitals with Matching

	Infant Baptism		First Communion		Confirmation	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	-0.0821**	-0.1159***	-.0812**	-0.1505***	-.0023	-0.0788
	(0.0317)	(0.0485)	(0.0338)	(0.0441)	(0.0465)	(0.0505)
Mean						
Dep. Var.	*10,665*	*7,347*	*8,945*	*7,176*	*6,335*	*5,317*
Matched		X		X		X
No. of obs.	4,3880	2,956	4,547	3,102	4,556	3,108

Note: The outcome variable is log of the labeled participation outcome. This provides the estimates when I match the treated and control dioceses using a set of covariates including, the number of first-time allegations, the diocesan Hispanic population, the total population, and the diocesan per capita income. Diocesan and year fixed effects are included. Standard errors are clustered at the diocesan level. Diocesan controls include percentage of diocesan population that is Hispanic and per capita income. Pre-scandal (2001) means of the dependant variables from high allegation dioceses are in italics. *** p <0.01, ** p <0.05, * p <0.1

Table A.10: Effect of High Allegations on Catholic Population and Parish Churches with Matching

	Catholic Population		Parish Churches	
	(1)	(2)	(3)	(4)
Treat × Post	-0.0741**	-0.1121***	-0.0813***	-0.0702***
	(0.0325)	(0.0409)	(0.0195)	(0.0217)
Mean				
Dep. Var.	*713,630*	*534,857*	*162.7*	*148.3*
Matched		X		X
No. of obs.	4,562	3,114	4,563	3,114

Note: The outcome variables are the log of the Catholic population and the log of the number of parish churches. Diocesan and year fixed effects are included. Standard errors are clustered at the diocesan level. Diocesan controls include percentage of diocesan population that is Hispanic and per capita income. Pre-scandal (2001) means of the dependant variables from high allegation dioceses are in italics. *** p <0.01, ** p <0.05, * p <0.1

Table A.11: Effect of High Allegations on Diocesan Schools with Matching

	Number of Schools		Student Enrollment	
	(1)	(2)	(3)	(4)
A. Elementary Schools				
Treat × Post	-0.1415***	-0.1731***	-0.1418***	-0.1712***
	(0.0327)	(0.0409)	(0.0362)	(0.0441)
Mean				
Dep. Var.	*71.6*	*61.2*	*22,050*	*18,290*
Matched		X		X
No. of obs.	4,539	3,106	4,541	3,108
B. High Schools				
Treat × Post	-0.1644***	-0.1888***	-0.1871***	-0.1658**
	(0.0380)	(0.0522)	(0.0564)	(0.0646)
Mean				
Dep. Var.	*7.02*	*6.09*	*4,001*	*3,522*
Matched		X		X
No. of obs.	4,108	2,898	4,109	2,898

Note: The outcome variable is log of the labeled participation outcome. This provides the estimates when I match the treated and control dioceses using a set of covariates including, the number of first-time allegations, the diocesan Hispanic population, the total population, and the diocesan per capita income. Diocesan and year fixed effects are included. Standard errors are clustered at the diocesan level. Diocesan controls include percentage of diocesan population that is Hispanic and per capita income. Pre-scandal (2001) means of the dependant variables from high allegation dioceses are in italics. *** p <0.01, ** p <0.05, * p <0.1

Table A.12: Key County Level Mortality Statistics for Men, aged 55 to 64, by Matched High and Low Allegation Dioceses in 2001

	High Allegation	Low Allegation
No. of Counties	546	670.2
Average Group Population	10,147	6,530
Average Total Population	212,855	115,311
Average Group Overall Mortality Rate	1,194	1,276
Number of Counties with Group Mortality Rates $\geq$ 10 per 100,000		
Suicide	17	14
Accidental Overdose	4	0
Liver Related	36	38

Note: The table shows key county level mortality statistics for the demographic group of men, aged 55 to 64, in 2001, one year before the abuse scandal. There is data for 2,919 out of 3,141 counties in the United States. All data comes from the compressed mortality files from the CDC Wonder database.

Table A.13: Effect of High Allegations on Incidence of Counties with Mortality Rate ≥10 per 100,000 for People Aged 55 to 64

	Male		Female	
	(1)	(2)	(3)	(4)
A. Accidental Overdose				
Treat × Post	0.0117*	0.0110	0.0072	0.0074
	(0.0069)	(0.0072)	(0.0045)	(0.0047)
Matched		X		X
No. of obs.	65,524	65,235	65,524	65,235
B. Suicide				
Treat × Post	0.0175**	0.0181**	0.0008	0.0010
	(0.0071)	(0.0071)	(0.0018)	(0.0019)
Matched		X		X
No. of obs.	65,524	65,235	65,524	65,235
C. Alcoholic Liver Disease				
Treat × Post	0.0200**	0.0157*	-0.0005	-0.0016
	(0.0082)	(0.0089)	(0.0035)	(0.0037)
Matched		X		X
No. of obs.	65,524	65,235	65,524	65,235

Note: The outcome variables are indicators whether a county experienced a mortality rate greater than 10 per 100,000 for each of the individual death categories. This provides the estimates when I match the treated and control dioceses using a set of covariates including, the county Hispanic population, the proportion of the county total population that is Catholic, and the county per capita income. Data comes from the CDC Wonder database for the years 1991-2015. County and year fixed effects are included. Standard errors are clustered at the diocesan level. County controls include percentage of population that is Hispanic, per capita income, manufacturing employment, and unemployment rate. *** p <0.01, ** p <0.05, * p <0.1

Appendix B

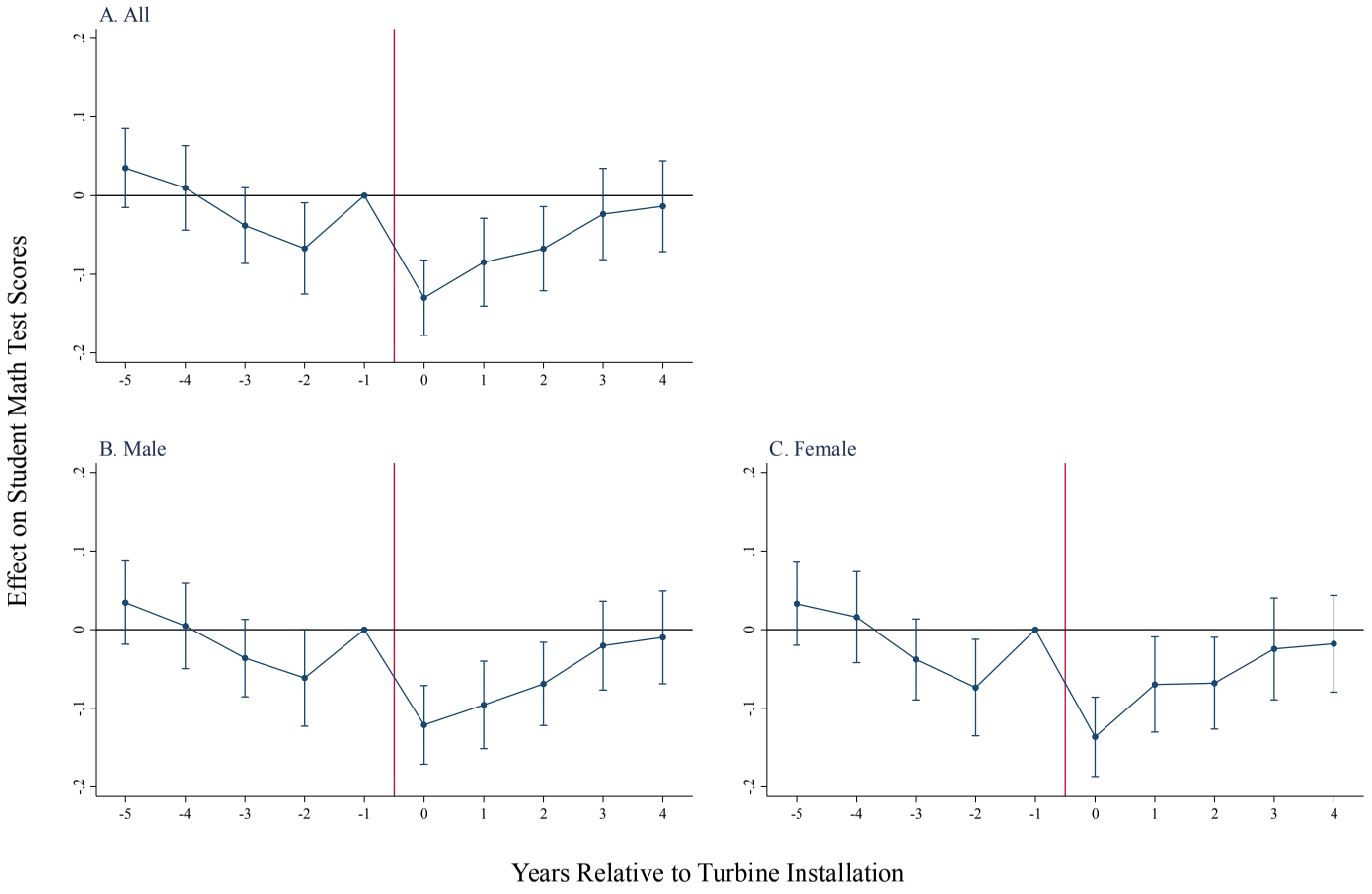
Disaggregated Event Study Plots

B.1 Standardized Math Scores

In a similar vein to the analysis in the main text, Figure B.1, Figure B.2, and Figure B.3 respectively plot the event studies for (1) male and female students, (2) white, black, and Hispanic students, and (3) gifted and at-risk students. Unsurprisingly, each of the subgroups follow the qualitatively same pattern of the event study for the aggregate effect on math scores shown in Panel A of each figure.

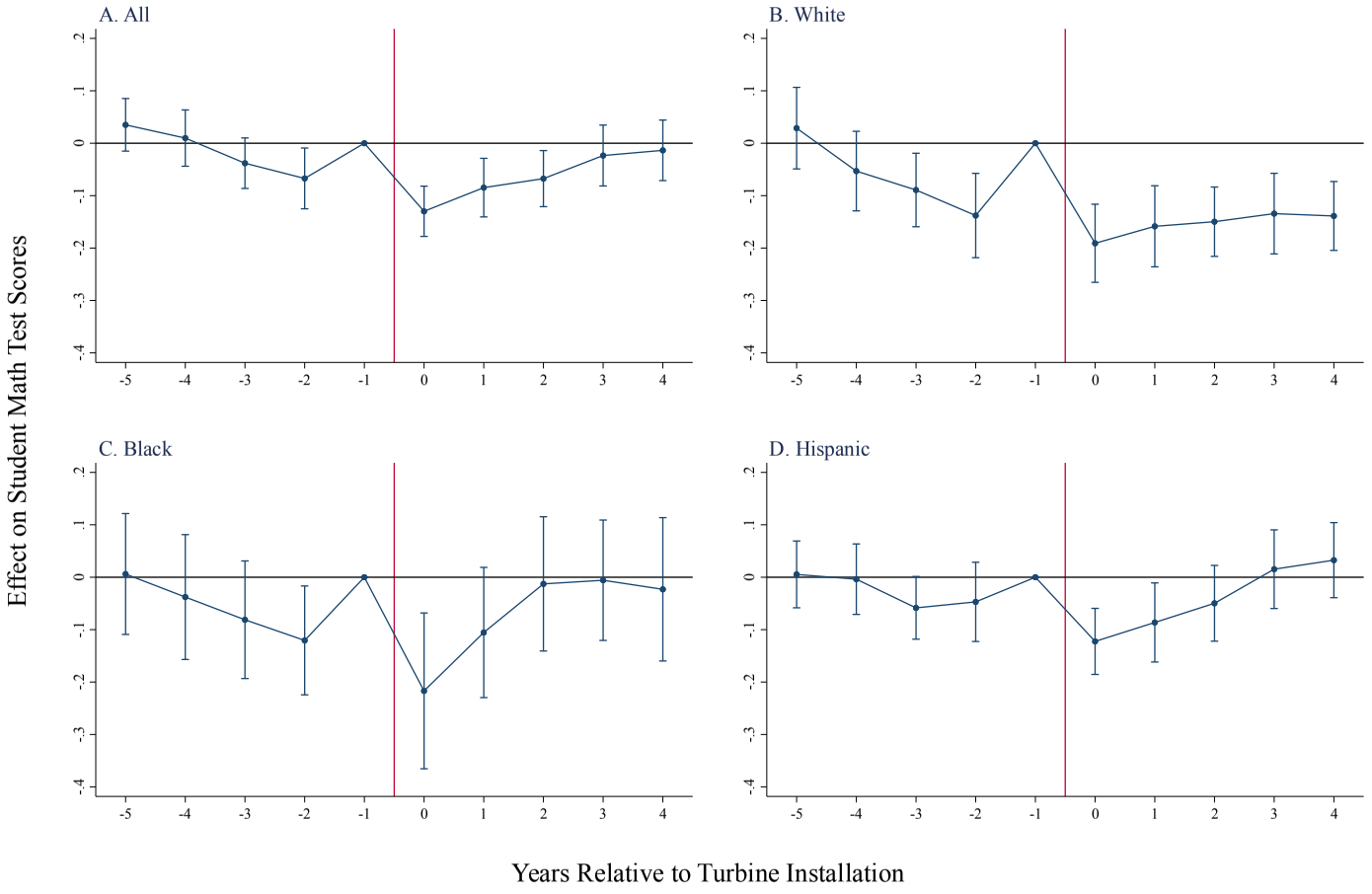
The plots in in Figure B.2 are interesting. First, the effect on white student math scores (Panel B) is -0.191 standard deviations and persists throughout the time line. Both Black (Panel C) students experience similar effects to that of white students in the year of installation. On the other hand, Hispanic (Panel D) student do not experience as large an impact in the initial year. In both of the subgroups, the negative effect dissipates within two years. The difference between the estimates on the pooled effect of white and Hispanic students, provided in Column 6 of Table 3.5, is quite large at 12 percent of a standard deviation and is significant.

Figure B.1: Effect of Wind Turbines on Math Scores by Gender within 10 km



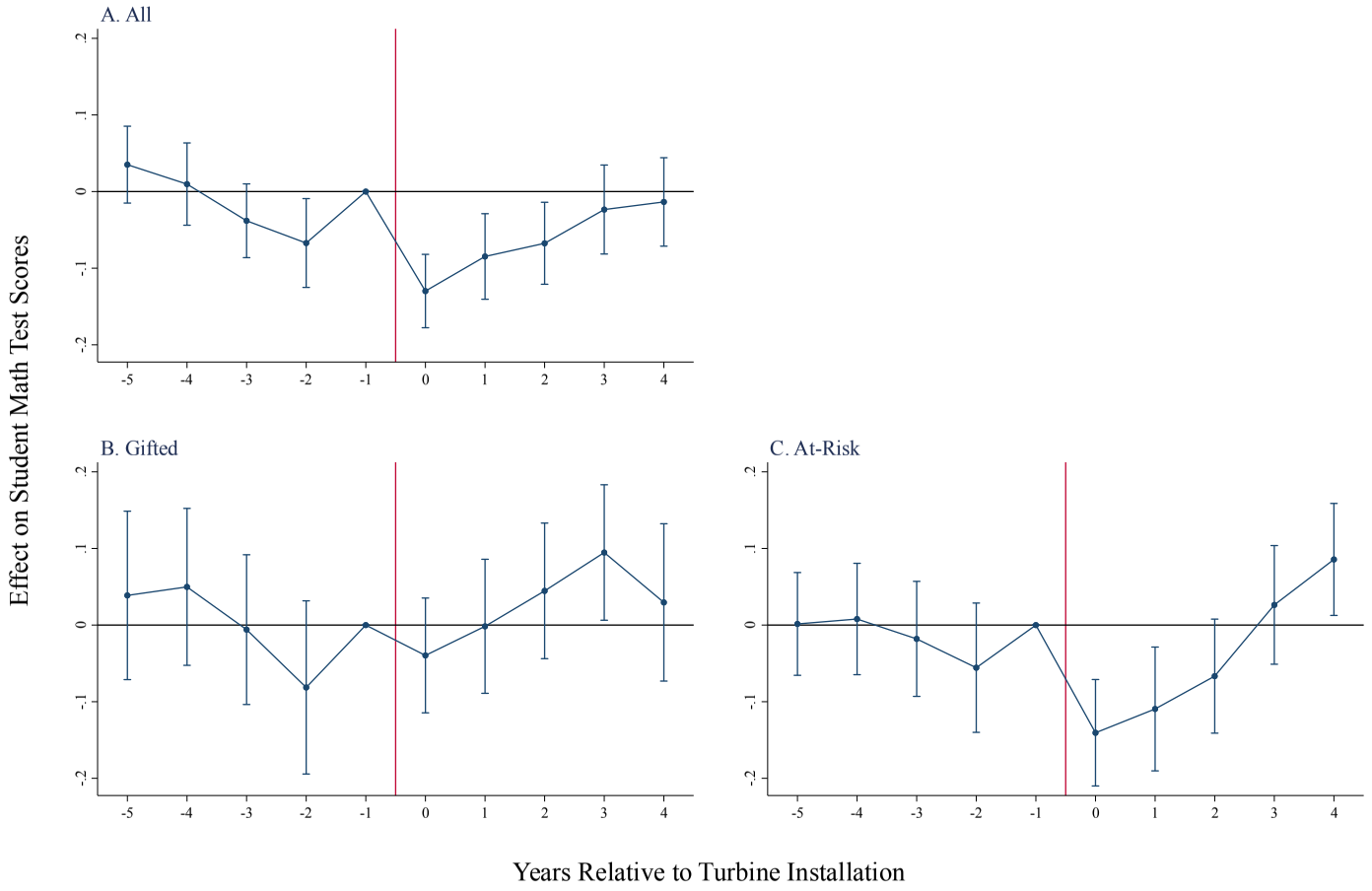
Note: Each panel provides the plot of the yearly coefficients from equation 3.2 with the z-scores from math standardized tests by male and female students. The solid vertical line is immediately before relative year 0 when the first turbine was installed. The capped vertical bars show 95% confidence intervals from standard errors clustered at the school level school.

Figure B.2: Effect of Wind Turbines on Math Scores by Ethnicity within 10 km



Note: Each panel provides the plot of the yearly coefficients from equation 3.2 with the z-scores from math standardized tests for the labeled subgroups. The solid vertical line is immediately before relative year 0 when the first turbine was installed. The capped vertical bars show 95% confidence intervals from standard errors clustered at the school level school.

Figure B.3: Effect of Wind Turbines on Math Scores by Academic Group within 10 km



Note: Each panel provides the plot of the yearly coefficients from equation 3.2 with the z-scores from math standardized tests for gifted and at-risk students. The solid vertical line is immediately before relative year 0 when the first turbine was installed. The capped vertical bars show 95% confidence intervals from standard errors clustered at the school level school.

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